THE

CYCLE OF CELESTIAL OBJECTS

CONTINUED AT THE

HARTWELL OBSERVATORY TO 1859.

WITH A NOTICE OF RECENT DISCOVERIES, INCLUDING DETAILS FROM THE

ÆDES HARTWELLIANÆ.

BY

VICE-ADMIRAL W. H. SMYTH, K.S.F.

D.C.L., F.R.S., F.R.A.S., ETC.

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Commonefacio.

Although it may not be *en règle* to prefix a formal dedication to a privately-printed book, my feelings insist that this astronomical fasciculus shall be sincerely addressed—as an expression of respect and regard—to my excellent correspondents

Adams, John Couch, Esq.
Airy, George Biddell, Esq.
Carrington, Richard C. Esq.
Dawes, the Rev. W. R.
De Morgan, Augustus, Esq.
Fletcher, Isaac, Esq.
Herschel, Sir John, Bart.
Hind, John Russell, Esq.
Jacob, Captain W. S.
Pogson, Norman, Esq.
Rosse, Earl of
Smyth, Charles Piazzi, Esq.
Stanhope, Earl
Wrottesley, Lord

with hearty thanks for their constant readiness to aid my inquiries. Indeed but for that, and the voluntary offer of our zealous and science-loving friend Dr. Lee, to be at the unconditional expense of printing it, this volume probably would not yet have been prepared by

Their faithful Servant,

W. H. Smyth:—

St. John's Lodge, near Aylesbury,
January 1st, 1860.

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THE "CELESTIAL CYCLE,"

CONTINUED AT THE HARTWELL OBSERVATORY.

INTRODUCTION.

(*Philosophy can*) "so inform
The mind that is within us, so impress
With quietness and beauty, and so feed
With lofty thoughts—that neither evil tongues,
Rash judgments, nor the sneers of selfish men,
Nor greetings where no kindness is, nor all
The dreary intercourse of common life,
Can e'er prevail against us."

WORDSWORTH.

MORE than a dozen years ago, I published a work under the title of "A Cycle of Celestial Objects," which brought our general astronomical knowledge pretty fairly up to the year 1844; but even in the brief span of time that has elapsed what a wonderful advance has been made, both in the theory and practice of that sublime science! Since then, the powerful meridian-instrument at Greenwich,—the monster telescope of Lord Rosse,—the multiplication of large refractors,—as well as the application of electro-magnetism to transfer the beats of a clock from one station to another, and to register celestial culminations—have been successfully accomplished. Never was the pursuit of Astronomy more liberally cultivated by the governments of Europe than during the time here cited, and our Colonial institutions have pulled a strong oar in the general movement; while in the United States of America, several public observatories have been so effectively established and worked, as already to have taken their

places in the front rank, as it were *per saltum*. Since the above mentioned book was launched, the zodiacal and slightly extra-zodiacal regions have been ransacked with such spirit and perseverance, that no fewer than fifty-two additional asteroids have been detected and harnessed; and the same period of triumph has witnessed the proudest result of astronomical theory and practice which the world has boasted since the time of Newton, namely, the prediction and discovery of the trans-Uranian planet, Neptune. Thus has science trod a brilliant career of energetic advancement in the domain of intellectual culture, diligently combining detail and generalization of the highest order; and, under the sure pilotage of sound induction, it has largely aided in the comprehension of the laws and order of the material universe. All this directly tends to advance our knowledge of the grander phenomena of Nature, on which successive ages will continue probably to throw progressive light; for, notwithstanding all the late harvests, we must not for a moment imagine that but little more is obtainable. In a word, promptness and accuracy in opening out the speculative faculties of man, form the distinctive glory of the age we live in; and, with astronomy as with all other branches of human acquirements, the discoveries already made, instead of exhausting the pursuit, should only act as allurements to vigorous energy and renewed hope.

From this grand and truly wonderful theme, I must now descend to the consideration of my own mite towards the general progress; since there appears to be a rational inducement that I should add another phase to my Cycle, as well to record the grand revealments that have dawned on the cultivators of astronomy, as to enrol the observations which I have made at Hartwell since the dismantling of the Bedford Observatory. We shall therefore commence at once with the Cycle's—

INTRODUCTORY SKETCH OF THE PROGRESS OF ASTRONOMY.

CHINESE ASTRONOMY.—On this subject (Cycle I. p. 4) I have further to add:—As the solar eclipse, for which the two state officers Ho and Hi—having

failed to predict it—were sentenced to death, is the most ancient observation of which any record has been preserved by History or Tradition, it is of the highest interest in the reminiscences of man as well as of science. I am therefore happy to avail myself of a correction and elucidation of this phenomenon which my late friend R. W. Rothman, Registrar of the London University, placed in my hands after his having read the statement that I have given, and found the authority which I had followed was upwards of forty years out in the date. By carefully scrutinizing the discussions of the Jesuit missionaries De Mailla and Gaubil, and by using better means and tables than they could obtain, Mr. Rothman satisfied every indication of the old Chinese account, and confirmed all* the particulars :—

"It appears," said my friend, "that a history called the *Chou-King*, a work of the highest authority, though its exact date is not known, contains a statement to the following effect: That, towards the beginning of the reign of the Emperor TCHOU-KANG, on the first day of the third moon of autumn, the sun was eclipsed in the constellation Fang. * * * * Another chronicle, the *Tchou-Tchou*, less ancient, though still of high antiquity—for it is anterior to the year 460 B.C., furnishes more particulars. It states that the eclipse in question took place in the fifth year of the reign of TCHOU-KANG, on the first day of the ninth moon; and it adds the cycle characters for the day and year, which correspond to the 13th of October, 2128 B.C.

As our able scholar justly recognised this to be the earliest existing muniment of astronomy, and saw its importance in chronology, he determined to ascertain, with the improved appliances of the present age, whether such an event really took place at the time specified. In the conclusion from these premises—startling as the antiquity of the observation undoubtedly is—he actually found all the indications of the Chinese historians completely verified—that a great eclipse occurred at noon-day on the identical 13th of October, 2128 B.C.; that the sun was in the sign Fang (*Scorpius*); and that the elements must have been as follow :—

* See the eleventh volume of the Memoirs of the Royal Astronomical Society. Mr. Rothman cites Tchou-King (Shu-Kang) as a *book*, whereas another good linguist interpreted it to me as a *person*. The names however may be easily reconcileable were it worth the while of a duly qualified person to rake among the Tsu-Shu, the Kang-Muh, the Ping-Hwan, the Hi-Kien, and the Ting-Wangs; but it is the eclipse which it is requisite here to keep in view, as a capital starting-point in the SCIENTIA SINICA.

	h. m. s.
Instant of true conjunction, Paris *m. t.* from midnight,	20 11 52.
True longitude of the luminaries	183° 37′ 39″.
True latitude ☽	+ 31 11.
Horizontal equatorial parallax ☽	55 59.
Horizontal semidiameter ☽	15 16.
Semidiameter ☉	16 16.
Horary motion ☽ in longitude	31 10.
" " latitude	2 53.

It further appears that the residence of the Emperor *Tchou-Kang* was a town of the third class, called *Tay-Kang-Kien,* in latitude 34° 7′ north, and longitude 40° east, from Paris, according to the missionaries. With these data he found that in mean time from midnight at that place there would follow :—

	h. m. s.
Instant of the greatest eclipse	12 8 47.
Least apparent distance of centres	3′ 41″.
Apparent semidiameter of ☽	15 28.
Magnitude of the eclipse, in digits	10·5.

so that a most important historical phenomenon is fully confirmed by modern science, after an interval of no less than 3986 years! This is a far more remote date than the one which I cited (vol. i. page 5), as the earliest of the Chinese observations on which the illustrious author of the Mécanique Céleste placed confidence: there is, however, a senior claim in the Brahminical Súrhya Suddánta *(sun demonstrated),* on which Laplace said—

"I find by my theory that, at the Indian epoch of 3012 years B.C., the apparent and annual mean motion of Saturn was 12° 13′ 14″, and the Indian tables make it 12° 13′ 13″ In like manner I find that the apparent and annual mean motion of Jupiter was, at that epoch, 30° 20′ 42″, precisely as in the Indian astronomy."

Moreover, I am told by a Pundit who is somewhat versed in these matters, that there is greater reliance to be placed on the ancient Chinese writings than on the Sanscrit, which latter are represented as being disfigured with interpolations, forgery, and falsehood! On this point authorities differ.

Eclipse of Thales (Cycle I. page 8). Since Mr. Baily devoted his talents to this historically and chronologically important eclipse, it has fortunately attracted the attention of my valued friend Mr. Airy, who, fully

armed in the panoply of modern improvements, advanced to the discussion with the research of an antiquary, the zeal of a scholar, and the ability of a mathematician. In an essay published in the Philosophical Transactions for 1853, he has attractively investigated the reported eclipses of Agathocles, Xerxes, and Thales, the objects being

"to explain the state of lunar astronomy, as bearing on the calculation of distant eclipses; to employ the eclipse of Agathocles as the cardinal eclipse from which the lunar elements were to be corrected, in order to obtain certain grounds for computation of the eclipse of Thales; and to effect that correction in a systematic way, by supposing the place of the moon's node liable to error, and exhibiting the change in the path of the shadow as a numerical multiple of that error."

Mr. Baily had arrived at the opinion, that only a total solar eclipse could satisfy the account given by Herodotus, and he made Agathocles land in the Lesser Syrtis, after passing through the shadow of one; but I afterwards convinced him that the disembarkation must have occurred at the Quarries, now called Alhowarreah, near Cape Bon, from a survey which I had made of that coast—thereby also confirming the statement of Diodorus Siculus, l. xx. c. i. He then expressed dissatisfaction with his Paper, and promised to return to it some day: but that day never came! Alhowarreah was adopted as the place of landing by Mr. Airy; though, from various moral and physical causes bearing on the question, I could not also agree with him either in supposing that the crafty Sicilian sailed through the Carthaginian fleet, braved Scylla and Charybdis, bearded the hostile Panormitans, and incurred a lengthy passage north of Sicily,—while there was a comparatively clear southern route. However, a supplement to that memoir was read to the Royal Astronomical Society in June 1857, in which Mr. Airy states:—

"Scarcely had the paper [*the essay above alluded to*] been given to the public, when I perceived that I had done wrongly in tacitly assuming all the lunar elements to be correct, except the place of the node. Mr. Adams published his doubts on the correctness of the received coefficient of the moon's secular acceleration; and the alteration which he proposed to make in its value was so large as materially to derange the calculations of ancient eclipses. About the same time the solar tables by MM. Hansen and Olufsen were received in this country, and the places given by them for distant ages differed sensibly from those given by Carlini's Tables (used in the investigations of 1852). It appeared, therefore, that the ground of my former calculations was completely taken away."

New computations of course furnished new results, and certain points of geography and military history were cleared from some of the looming which had obscured them. After a learned and scientific re-examination of the conditions, to which we refer all astronomical antiquaries, Mr. Airy now brought the route of Agathocles to the *south* coast of Sicily, and the eclipse which then happened as occurring on August 14, 18^h 40^m (*Greenwich mean solar time*), B.C. 309. Similar reasonings and elements gave May 28, 4^h, 0^m B.C. 584, as the chronological point for the oft-discussed eclipse predicted by Thales.

THE DISCOVERY CLAIMED BY PYTHAGORAS (Cycle I. page 10). The Chinese records are said to show, so far back as B.C. 1100, that it was known by Tchou-Kang that the square on the hypothenuse of a right-angled triangle is equal to the sum of the squares on the other two sides: thus anticipating Pythagoras by upwards of five centuries in the discovery which he valued at a hecatomb. Still, there cannot exist a single doubt of his having brought it to light independently.

THE GIRALDA TOWER (Cycle I. page 31). To the mention of Geber, or Guëver, it should have been added, that the invention of Algebra has been erroneously ascribed to him. Yet not only was he one of the best astronomers and chemists of his age, but he must also have been a good architect; for the celebrated Giralda tower, now the belfry of Seville Cathedral, was built under his superintendence in 1196, and used as an observatory of the heavens —being perhaps the oldest monument in Christendom consecrated to science. So solid is this building, that my old friend Don Sanchez Cerquero* assured me it was still quite free from tremors, so that it would bear meridian instruments, were the upper part adapted to that purpose. It is altogether an interesting structure, and those who spared it when they destroyed the mosque

* This officer led a chequered life. He was a lieutenant in the Spanish Navy in 1810, when we both commanded gun-boats at the siege of Cadiz. He afterwards became Director of the Royal Observatory of San Fernando, near that city, and was thus again in correspondence with me. Finally he sought refuge from trouble and political broils in a Capuchin's cowl, and died in 1850. He was an elegant scholar, and well acquainted with the Spanish, English, French, Italian, and German languages and literature.

where the proud cathedral now stands, not only displayed taste, but also discrimination, in saving an edifice which has been equally useful and ornamental to the city. It towers above all its compeers far and near, and is surmounted by the Giralda, a brazen statue of Faith, whence the tower derives its name, for, although it weighs two and a half tons, it turns with the lightest winds.

NICHOLAS COPERNIK (Cycle I. page 37). Speaking of the ancients, upon whom Copernicus based his celestial hypothesis, he is represented as having consulted Aristarchus: but in the supplement to the seventh edition of the German *Conversations Lexicon, art. Astronomie*, it is stated that Copernicus was unacquainted with the opinion of Aristarchus, as related by Avenarius (*John Haberman*), the last-mentioned author's work not having been printed till some time afterwards.

EARLY ENGLISH SCIENTIFIC WORTHIES (Cycle I. page 40).* Among the names selected in illustration of our claim to philosophical rank in the middle ages, we ought to have recollected Richard Rollo, a learned Eremite of the order of St. Augustine, who, from his long residence at the priory about four miles from Doncaster—where he died in the year 1349—was commonly called *Richard de Hampole*. In describing the joys of heaven in his "Stimulus Conscientiæ," he gives, as a necessary preliminary, the actual situation of the celestial abode according to the astronomical theory of that period, based on the Ptolemaic system; showing that the "sterrid" and the "cristalle hevens" roll around us, and "schal never cese til domesday:"—

"For if hit stode never so short time stille,
All that es in erthe schold perische and spille.
Thus telleth the clerks of clergie
That haveth lerned of Astronomye."

Richard describes the seven "planetis that bethe aboute us," and above them the stars ever turning round as the nails on a wheel, each of them in

* On the said page 40, line 13, is a slight typical error: 1068 has been printed for 1060; but it is corrected in the list which follows.

size "es more than al the erthe." This was a pretty fair estimate for his day; and his notion of their distance *(apud Sidrac)* is certainly more exalted than was the demonstration of Leonard Digges, his junior by two centuries and a quarter. But mark our Richard, and note how greatly his weighty rock beats the nine-days' whirling anvil of Hesiod:

> "And to the heghe heven couthe never clerk by eny art
> The space gesse by a thousand part,
> For hit es so heyghe, Sidrac saith, in his menying,
> That if a stone were there at with beyng
> And were of an hundred mennes lyfting,
> Yut hit schuld be in down fallyng
> A thousand yere and nought one lasse,
> Or that hit myght al the hevens passe."

Allusion to Hevelius (Cycle I. page 48). In mentioning the Catalogue of 1564 stars, produced by Hevelius, as being inferior to the more modern ones, it was rather with the recollection that he obstinately adhered to the use of *pinnacles* in making his observations, than to any deficiency of zeal or skill in the observer. He was blind to the advantages of the telescopic sights then in use elsewhere, though, when we consider the state of practical astronomy in that day, he was really a surprising man; and in all comparisons of the kind De Morgan's excellent advice should be borne in mind:—

"The reader who is unaccustomed to think of scientific history may transfer more modern views to the credit of older systems, and may not be able to learn that names which are now unknown to general fame are essential to a sufficient view of history; and in both these errors he may receive some encouragement from many who ought to know better. But there is one still greater error which he will actually learn from the writings of the best historians, and from the conversation of those who are best qualified to read the histories: namely, to judge the merit and demerit of a former age by the comparison of their methods with our own, instead of with the methods of those who went before them. No one is so conspicuous a teacher of this folly as Delambre, the greatest of astronomical historians; but the fault is that of his time. We are in the midst (let us hope near the culmination) of a long reaction consequent upon the long period of excessive reverence for antiquity. The nineteenth century will be known in history as the most uplifted of the self-glorifying centuries; and those of a remote time, to whom the difference between the sixteenth and the nineteenth centuries will, as viewed from a distance, not seem quite so great as to us, will be amused by our crowing."

Captain Halley (Cycle I. pages 54 and 55). My mention of this very distinguished philosopher's being an acting-captain in the Royal Navy, has been carped at by one more addicted to controversy than to argument, who insists that he was only a "sort of passenger in a hired vessel, for the Paramour never belonged to our Government." His grounds for assertion I know not, but in Charnock's "History of Marine Architecture" (vol. ii. page 435) the Paramour pink, fifth rate, appears in the list of King William's fleet. However, to settle the point beyond idle dispute, I last year obtained through Captain Washington, Hydrographer of the Navy, sufficient proof that Halley acted under a Royal Commission, as I have stated; and all the details are proved by the following documents, extracted expressly for me *verbatim et literatim* from the Minute-Books of the Admiralty.—First, his instructions, dated 16th October, 1698.—

"Whereas his Majesty has been pleased to lend his Pink the Paramour for your proceeding with her on an expedition to improve the knowledge of the Longitude and variations of the Compasse, which shipp is now completely Man'd, Stored, and Victualled at his Majesty's Charge for the said Expedition: You are therefore hereby required and directed to proceed with her according to the following instructions:—

"You are to make the best of your way to the southward of the Equator, and there to observe on the East Coast of South America, and the West Coast of Affrica, the variations of the Compasse with all the accuracy you can, as also the true situation both in Longitude and Latitude of the Ports where you arrive.

"You are likewise to make the like observations at as many of the islands in the seas between the aforesaid Coasts as you can (without too much deviation) bring into your Course; and, if the season of the year permit, you are to stand soe farr into the South till you discover the Coast of the Terra Incognita, supposed to lye between Magolan's Streights and the Cape of Good Hope, which Coast you are carefully to lay down in its true position. In your return home you are to visit the English West India Plantations, or as many of them as conveniently you may, and in them make such observations as may contribute to lay them downe truely in their Geographicall Scituation. And in all the Course of your Voyage you must be carefull to omit no opportunity of noting the variation of the Compasse, of which you are to keep a Register in your Journall.

"You are, for the better lengthning out your Provisions, to put the men under your command when you come out of the Channel to six to four men's allowance, assuring them that they shall be punctually pay'd for the same at the end of the voyage.

"You are during the Term of this Voyage to be very carefull in conforming yourself to what is directed by the Generall Printed Instructions annexed to your Commission, with regard as well to his Majesty's honor, as to the Government of the Shipp under your Command. And, when you

return to England, you are to call in at Plymouth, and, finding no order there to the contrary, to make the best of your way to the Downes, and remaine there till further Orders: Giving us an Acc[t] of your arrivall."

This document, so conclusive of Halley's full command, is duly signed by the Lords of the Admiralty, countersigned by their well-known Secretary Mr. Burchett, and addressed to "*Captain Edmund Halley, Commander of His Majesty's Pink the Paramour.*" And respecting the mutiny for which his lieutenant was cashiered, as asserted in the Cycle (I. page 55), a full proof is given in the order sent by the Admiralty to "*Sir Cloudesley Shovell, Knt. Admiral of the Blewe, Downes.*" This is an exact copy of that document:—

"Whereas wee have rec[d] a Letter from Capt[n] Halley, Comand[r] of his Majestie's Vessell the Paramour Pink, complaining of Lieut. Harrison, the officer which acts as mate and Lieut. of the said Pink, a Copy whereof comes inclosed, which complaint Wee think fitting should be inquired into at a Court Martiall upon her arrival in the Downes: You are therefore hereby required and directed to cause the same to be strictly inquired into and tryed at a Court Martiall accordingly, for holding whereof you are empowered by our late Warrant to you.

"And, when the Court Martiall shall be over, you are to order the Coñander of the said Pink forthwith to repaire with her to Long-reach, and there hasten the putting ashore her Guns, Stores, and Provisions, and from thence to proceed to Deptford, where she is to be paid off and laid up.

"Dated &c. the 29th of June '99.

BRIDGEWATER.
HAVERSHAM.
ROBERT RICH.
GEORGE ROOKE.
DAVID MITCHELL.

"By command, &c.
JOSIAH BURCHETT."

By the Paramour's being paid off after the ringleader of the mutiny was broke, an opportunity was afforded of getting rid of all her malcontents; but, so far from being "laid up," she was again placed under Halley's command, and in September of the same year, 1699, he proceeded a second time on the mission of Magnetism, Longitude, and Maritime Discovery. No doubt then can possibly remain of his naval commission; but the above evidence may be clinched, and the value of Halley's scientific acquirements shown, by another official document, which the Admiralty sent him when he had completed his Atlantic voyage:—

"Whereas his Majesty's Pink the Paramour is particularly fitted out and Putt under your Command that you may proceed with her and observe the course of the Tydes in the Channell of

England, and other things remarkable: You are therefore hereby required and directed to proceed with the said vessell, and use your utmost care and Diligence in observing the course of the Tydes accordingly, as well in the Midsea as on both Shores. As alsoe the Precise times of High and Low Water, of the Sett and Strength of the flood and Ebb, and how many feet it flows, in as many and at such certain places as may suffice to describe the whole. And whereas in many places in the Channell there are Irregular and halfe Tydes, you are in a particular manner to be very careful in observing them.

"And you are alsoe to take the true bearings of the Principal head Lands on the English Coast, one from another, and to continue the meridian as often as you conveniently can from side to side of the Channell, in order to lay downe both coasts truely against one another.

"And in case during your being employed on this service any other matters may occur unto you, the observing and Publishing whereof may tend towards the security of the navigation of the subjects of his Majesty, or other princes trading into the Channell, you are to be very carefull in the taking notice thereof; and when you shall have performed what service you can, with relation to the particulars before-mentioned, you are to returne with the Ship you command into the River of Thames, giving us from time to time an account of your Proceedings.

"Dated this 12 June, 1701.

PEMBROKE.
DAVID MITCHELL.
GEORGE CHURCHILL.

"By command &c. J. BURCHETT."

To Captⁿ Edmᵈ Halley,
Comdʳ of his Majesty's
Pink the Paramour.

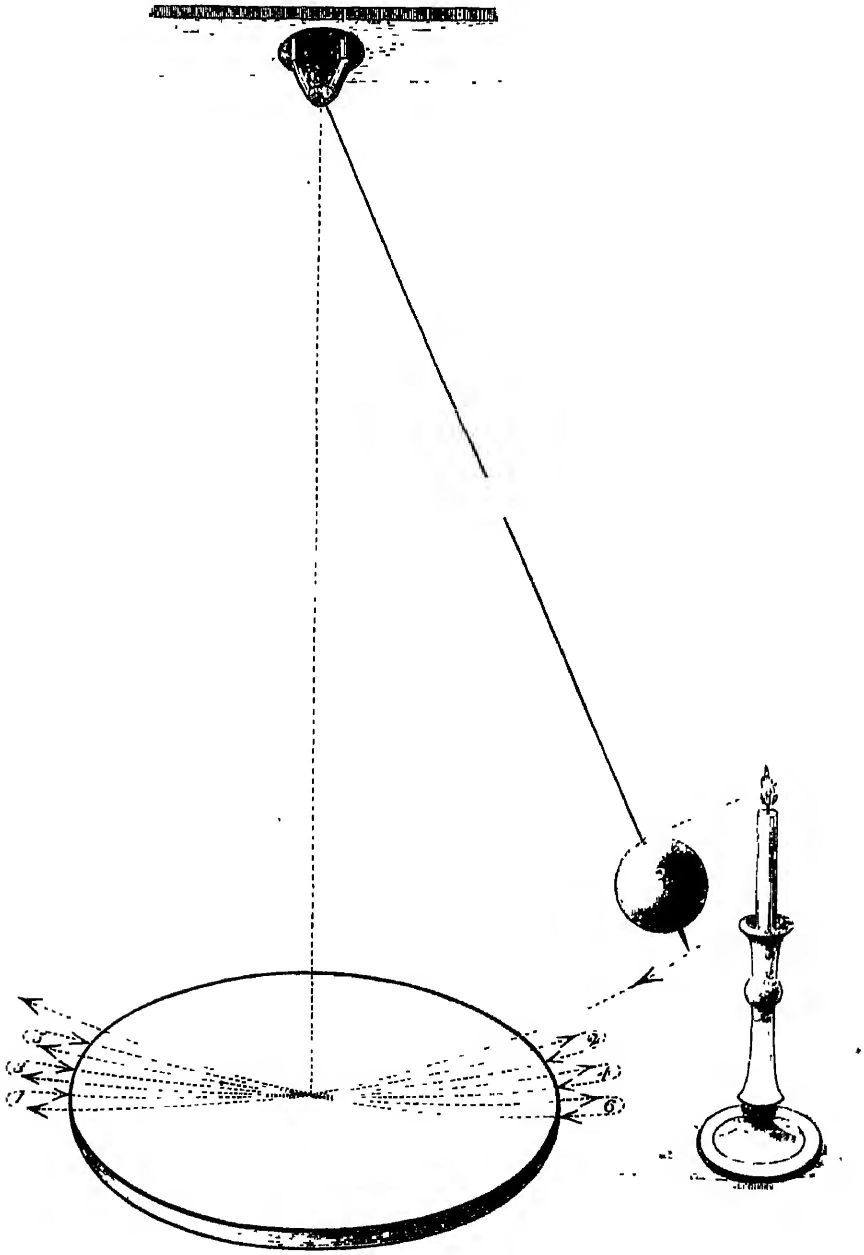
THE EARTH'S ROTATION ON ITS AXIS (Cycle I. page 63). Since this statement of the impossibility of the Earth's being in absolute repose was published, we have all been most unexpectedly delighted with a material chamber-demonstration of the diurnal rotation of our globe—an acknowledged fact, yet hitherto baffling to the senses—by my friend M. Léon Foucault. This result of profound conception, and ingenious experiment, was reduced to easy practice by his long pendulum: for, although Laplace did publish the principle in a few words, he at the same time stated, that the effect could not be rendered manifest on account of accompanying phenomena, which would overpower the weak influence of the earth's rotation. It is true that there is another cause present, namely, elliptical motion, which will also make the pendulum's plane alter, and we have to ascertain the amount of each. The nature of the elliptical path which the pendulum will be found to describe must be used as a correction,

according to its direction and the latitude of the site, to the rate of change of plane, and the residue will give the earth's rotation. Therefore, unless this process when applied to the whole change in the Pendulum's plane leaves exactly 360° in 24 hours, which is the assumed rate of our rotation, we have not thereby confirmed it.

In this adaptation M. Foucault has not been allowed to walk the course. A claim was started for the Academicians *del Cimento,* who had made numerous observations on the motions of the pendulum, in and about 1660, but it seems that the anomalies which they detected were not supposed to have any connection with terrestrial rotation. And in a Paper of the Philosophical Transactions for 1742 (No. 468) by the Marquis Poli, in the course of some observations on a pendulum of a different kind, the author remarks—"I then considered (adopting the hypothesis of the earth's motion) that in one oscillation of the pendulum there would not be perfectly described from its centre one and the same arc in the same plane:" still he did not perceive, probably from the imperfection of his means, how nearly he had gained the truth, for reasoning of a higher order, conjoined with the accumulated mathematics of ages, require to be called in to assist raw phenomena.

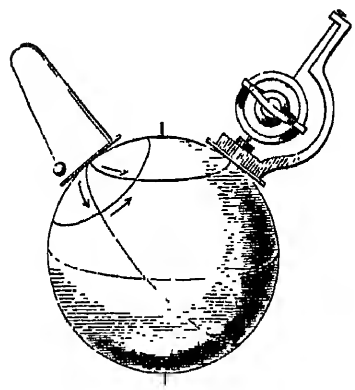
The resulting experiments are based on the principle, that the direction of a pendulum's plane of vibration is not affected by any cosmical motion of translation which may be given to its point of suspension; let us therefore consider an extreme case. If a pendulum, suspended any where so near the pole of the earth that the circle around it may be considered a plane, be put in vibration on a plane passing through the pole, this plane, continuing parallel to its original direction, as it is carried by the earth's rotation round the pole, will make a varying angle with the line drawn to it, from the new position. After having thus gone through a quarter of a revolution, it will make an angle of 90° with the line first drawn to the pole, and so on. In confirmation of this assertion it will be found, all else remaining the same, that there will be no such effect if the experiment be tried at the equator instead of the pole. Now M. Foucault perceived the result which must ensue on the pole's appearing

to be carried round the plane of the pendulum's vibration: he reasoned on the resolution of the rotatory motion of any point of the earth's surface into two motions, namely, one round that point, and the other at right angles to the former, which would not affect the plane of the pendulum's vibration, although the first would do so. Well might εὕρηκα have burst from his lips—for thus



with an instrument easily carried from place to place, he has rendered what was a perplexing problem now sensible to the eye by our own fireside: since the results being the direct effect of terrestrial rotation, they supply a palpable proof of that long-contested motion, without reference to objects beyond the limits of our globe.

A year and a half after the successful exhibition of the pendulum, M. Foucault presented to the Academy of Sciences another convenient chamber-method of revealing the earth's rotation, by a mechanical method. The gyroscope consists of a very thick metallic disc pierced by an axis, the ends of which are supported on gymbals in a brass circle, and that is again similarly sustained in an outer circle by pivots at right angles with the axis first mentioned; in short like a marine barometer, only that the exterior ring is suspended by a thread without torsion. When the heavy metallic disc is made to revolve with great velocity, its axis preserves its direction unaltered, despite the earth's rotation, until the disc relaxes its speed, and during that interval it is easy to measure the apparent change owing to the translation of the point of suspension. The perfect balancing of this instrument however is very difficult, and so much depends on the due adjustment of the screws, that it is by no means a proof of the *time* required for the complete gyration of our globe, as the apparent period depends on that adjustment. Nor is it either a new apparatus entirely, or a new application of it, as Mr. Sang of Edinburgh fully described and published the one and the other eighteen years ago, although the practical accomplishment was not carried out for want of funds.



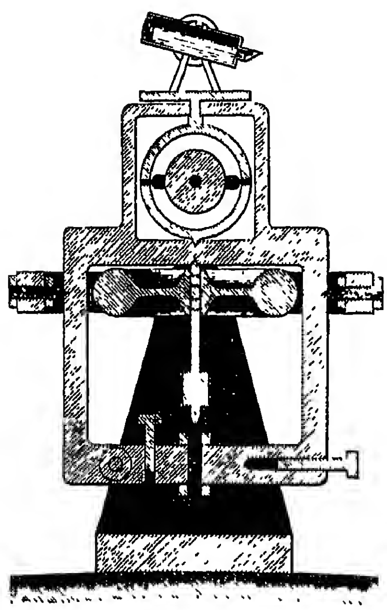
Moreover, with the exception of some refinements, the gyroscope in its present form has been in use during 50 years to illustrate *precession*, and there is still in Edinburgh the original instrument presented by Laplace for that purpose to Professor Playfair, whose successor, Sir J. Leslie, used it regularly for many years in his Lectures on Natural Philosophy.

As far back as 1752 a paper was read by Mr. James Short to the Royal Society of London, describing Mr. Serson's "horizontal top," which continued to spin full half an hour, and whose "upper side, which is polished, about two minutes after it was set up, sustained itself in such a manner as to offer a true horizontal plane; and that this plane was not disturbed by any motion or inclination you give the box in which it is placed, and therefore might be proper to be used aboard a ship; by which means seamen might be enabled to take the altitude of the sun or stars, in order to find their latitude, even though they cannot see the horizon in thick or hazy weather." But the inventor was unfortunately lost in the Victory, 110 guns, before he could perfect his labour.

This experiment was repeated about the year 1820, by our celebrated mechanist, Edward Troughton; still, as the supporting pivot partook of the irregular movement of the ship, with other disturbing forces, it proved a failure, despite many clever appliances. Accordingly, when I returned to England in 1824, on inquiring of my old friend as to the result, he said in his usual terse manner, "Ah, I thought I was making a good artificial horizon, but it turned out to be only a spinning-top!"

In 1845, my son, Professor C. Piazzi Smyth, being then on his homeward voyage from the Cape of Good Hope, imbued with the full importance of astronomical observations by ten years' experience in that observatory, tried various means of obtaining an artificial horizon at sea, and of eliminating the angular motion of the ship. In 1846 he made practical acquaintance with Troughton's fruitless application of rotatory motion to the same purpose. But from the year 1850, when he began a course of Lectures on Astronomy in the University of Edinburgh, including Navigation, Professor Smyth set himself to work, as a duty, to endeavour to improve the instruments, and to facilitate the use of them. He accordingly, step by step, came to an opposite application to that adopted by M. Foucault, of the rotatory principle, and achieved an apparatus for giving an invariable plane to a platform for astronomical instruments to be used at sea—and a precious boon this would be to navigation! He defines it as his *Compound-free-revolver-stand,* based on the simple dynamical

law of the tendency of a body in rotation to retain that movement in the same plane, if perfectly balanced, irrespective of the motion of external objects: in other words, that a body revolving round one of its axes of permanent rotation, free from disturbances, will preserve its plane of rotation unaltered.* I subjoin a sectional view of this stand,—a further description of which will be found in the inventor's Essay on the *Angular Disturbances of Ships*, printed in the Transactions of the Royal Scottish Society of Arts (vol. iv. part iv.)



There has however been no little confusion in the public mind on this head, by not making a proper distinction between the Foucault gyroscope and Piazzi Smyth's Free-revolver-stand—the former being constructed expressly to show motion in the rotation of our globe,—while the latter is intended to counteract and neutralise every irregular disturbance of the ship or other substratum. The Professor, by taking advantage of the permanence of its direction in space which a rotating body strives to maintain, enables us to take the necessary observations at sea, independent of the pitching and rolling and scending and yawing of the vessel amid the waves; and this very important result he has shown to be practicable in his paper entitled "Experiences with a *Free-Revolver-stand for a Telescope at Sea*," made during a voyage to Teneriffe in the summer of 1856. He had the satisfaction of being the first man who in a troubled sea looked through a telescope steadied by the same mechanical agency as that which preserves

* Sir John Herschel, in his Outlines of Astronomy (1858), describes an elegant experiment with an 18-inch globe, by which the mechanical fact upon which the whole theory hangs may be rendered evident to the senses:—if the globe be taken out of its wooden stand, and held with both hands by the brass meridian in a horizontal position, no additional effort is required, though the globe be spun rapidly on its axis, until an endeavour is made to change the direction of the axis, when a considerable resistance will be felt. This was successfully proved on my visit to Mr. Isaac Fletcher, at Tarn Bank, Cumberland, in the same year.

the constancy of the direction of the earth's axis in space. But let him speak for himself:—

"Having performed these adjustments, and brought the horizon of the sea into the field of view, I was delighted to find it remain there, absolutely uninfluenced by the rolling and pitching of the yacht; nay it even remained bisected on the wire, sufficiently long for the captain, the first and second mates, and several of the sailors to look in and bear witness of the fact. They saw this consummation long desired at sea, and they took kindly to the instrument, though it was an innovation on nautical practice.

"In order to perfect the balance of some parts of the machine, which had left its maker's shop rather unfinished, I kept the sailors for several days working the driving wheels while I was experimenting; they laboured at them with enthusiasm, indeed rather too much so, for one unfortunate evening, when the instrument was so nearly perfect, that I hoped in the course of the night to essay the grand nautical problem of observing an eclipse of one of Jupiter's satellites, the sailors, impatient of getting up immediately the full speed, clapped on a rope to the handle of the driving wheel, and pulled away in such style, that they twisted the strong steel driving axle in two, and were themselves laid flat along the deck."

Moreover, that it may not appear to be left a one-sided argument, I will here subjoin a passage from the Astronomer Royal's Report addressed to the Admiralty, in answer to the Council of the Royal Scottish Society of Arts:—

"On the general principle I have to remark that it is theoretically correct. In the application, various small practical difficulties have been met and overcome. The apparatus has been tried by Professor Smyth in his voyage to Teneriffe in a small ship; and as test of steadiness, a telescope carried by the platform was directed to the sea horizon, and a wire in the field of view was brought to the horizon while the ship was in violent motion. From my acquaintance with the theory of the construction, and from the experience I have had in handling the models first made (which when the fly-wheel is in rapid rotation resist all change of inclination with a pertinacity which appears magical,) I do not doubt that this statement is strictly accurate.

"I attach little importance to the mechanical difficulty which I have marked ii. (*the apparatus being above the ship's centre of gravity*): I have no doubt that easy and inexpensive trials of apparatus will master it.

"It will perhaps be necessary for supplying the want marked *a* (*means of observing altitudes with an invisible horizon*), and will certainly be necessary for *b* (*using the azimuth-compass*), and *c* (*observing Jupiter's satellites*), that means should be contrived for counteracting the yawing movement of the ship. It is sufficiently obvious that adopting a repetition of the same principle but in a different form, a fly-wheel whose plane is vertical, may be used to check the irregularities of horizontal yawing. I am informed by Professor Smyth that he has tried the combination of horizontal fly-wheel and vertical fly-wheel in model, and has found it to answer quite well, but it has not yet been tried at sea."

Nor is this all, for in the Essay alluded to above on the "Angular Disturb-

ances of Ships" there is actually a sectional elevation of a *Pressure-stand*, which may presently appear, and thereon is a gentleman sitting as comfortably at his telescope as he would be on shore. This would be real progress after all the marine-chairs, swinging-tables, and other pendulum-atic modifications that have successively been tried and proved worthless: while the very vision of such future facilitations in nautical astronomy, induces the thought that I betook myself to sea-life a century too soon.

Recent cultivators of Science (Cycle I. page 68). The very splendid muster-roll which is there given of the Newtonian School, being still considered rather deficient, we will now supply the following authors, as having borne a distinguished part in the successful pursuit of knowledge; and, as before, will append a note of their principal works. My former list extended from the death of Newton in 1727, to the year 1840, since which date the hand of death has dealt some heavy blows on the scientific circle; for besides a numerous corps of meritorious though rear-rank votaries, who have fallen victims to the "Fell Serjeant," the grave has closed over a goodly array of leaders, depriving me of many valued comrades and correspondents. Proceed we then to the additional enumeration.

1728. Reyneau, *Mathematician. Analyse démontrée. Science du Calcul des Grandeurs.*
—— Daniel, *a learned French Newtonian. The World of Descartes; a satirical confutation of the Cartesian Philosophy.*
1731. Taylor, Brook, *Mathematician. Centre of Oscillation. Solution of the Problems of De Moivre and Leibnitz. The celebrated Theorem which bears his name. Perspective.*
1732. Feuillée, *Mathematician, Astronomer, and Botanist. Observations Physiques. Researches and operations in Geography and Navigation.*
1736. Fahrenheit, *Philosophical Experimenter. Dissertation on Thermometers. Specific Gravity of Bodies. Graduation of a standard Thermo-scale.*
1739. Saunderson, *the blind Professor of Mathematics at Cambridge. Treatise on Fluxions. Explanations of the Newtonian Philosophy.*
1740. Kirch, *Astronomer. On Comets; Occultations of Venus and Mars; and on new Stars.*
1741. Gravesande, *Mathematician. The Newtonian Philosophy. Elements of Algebra, and other Works. Construction of Pneumatical Engines.*
1744. Beighton, *Mathematician. Improvements in the Barometer. Tables for Surveying.*
—— Desaguliers, *Experimental Philosopher. The Newtonian System. Catoptrics. Dioptrics. Electricity. Mathematical Elements of Moral Philosophy.*

1748. Bernoulli, John. *On the Elliptical Figure of the Planets. Recueil pour les Astronomes.*

1749. Jones, William, *Mathematical Navigator. Palmariorum Matheseos. Quadratrix to the Circle. The Relations of Goniometrical Lines, &c.*

1750. Crousaz, *Mathematician. Géometrie des Lignes et des Surfaces Rectilignes et Circulaires.*

—— Bilfinger, *Philosopher. Elementa Physices. De Speculo Archimedis.*

—— Jurin, *Mathematician. Fluxions. Distinct and Indistinct Vision. Defence of the Newtonians.*

1751. Robins, *Mathematical Engineer. Certainty of Newton's method of Fluxions. Treatise on Quadratures. Theory of Motion. New Principles of Gunnery.*

—— Machin, *Astronomer. Laws of the Moon's Motion. On the Curve described by a descending Body. Various essays on Philosophical Inquiry.*

1752. Cramer, Gabriel, *Mathematician. Theory of Curved Lines. Elementa Universæ Matheseos.*

1753. Berkeley, Bishop. *A new Theory of Vision. Defence of Free-thinking in Mathematics.*

1754. Folkes, *Physicist and Antiquary. On Microscopes. On Mock Suns. The Aurora Borealis.*

—— Wolfe, *Philosopher. De Algorithmo infinitesimali differentiali. Elementa Matheseos Universæ. Associate in the Acta Eruditorum. Aerometry.*

1758. Nicole, *Mathematician. Calculus of Finite Differences. Irreducible Case in Cubic Equations.*

1760. Catlyn, *Mathematician. Calculations of the Transit of Mercury. Observations of a Lunar Eclipse. Various scientific Essays.*

—— Colson, *Mathematician. Fluxions and Infinite Series. Negative-Affirmative Arithmetic.*

—— Hayes, *Philosopher. Fluxions. Conic Sections. The Moon-Chronology. Longitude.*

1761. Musschenbroeck, *Physicist. Magnetism. Natural Philosophy. Elementorum Physico-Mathematicorum. Compendium Physicæ Experimentalis.*

1765. Corsini, Edward, *Mathematician and Antiquary. Geometrical Elements. Philosophical Institutions. A Course of Metaphysics.*

1766. Anich, Peter, *Astronomer, Geometrician, Chorographer, and Mechanician.*

1767. Abauzit, *a friend and correspondent of Newton. General Physics. On Music.*

1768. Lisle, *Astronomer. Memoirs on Astronomy, Geography, and Physics. On Jupiter's Satellites.*

—— Simson, *Mathematician. Sectionum Conicarum. Edition of Euclid. On Porisms.*

—— Smith, R. *The Philosophy of Musical Sounds. Astronomy. A Complete System of Optics.*

—— Stone, *Mathematician. Method of Fluxions. Conic Sections. Figure and Magnitude of the Earth. Parallaxes. The Elements of Euclid.*

1769. Auteroche. *Nautical Astronomy. Trials of Le Roy's Time-keepers. Astronomical Tables, &c.*

—— Chaulnes, Duc de, *Astronomer and Mathematician. On the distance of Arcturus from the Sun's limb at the Summer solstice. Improvements in Chemistry.*

—— Passemant, *Optician, Improver of Telescopes and Microscopes. A six-foot reflector to equal a refractor* 150 *feet long. Constructor of an Astronomical Pendulum.*

1771. Bevis, John, *Astronomer. Lunar and Solar Eclipses. Venus, Mercury, Moon, &c.*

—— Rowning, *Mathematician. The Fluxionary Method. Roots of Equations. Mathematical Demonstrations. System of Natural and Experimental Philosophy.*

—— Rutherford, *Natural Philosopher. Mechanics. Optics. Hydrostatics. Astronomy.*

1772. Canton, *Physicist. Electrical Experiments. Artificial Magnets. Transit of Venus in* 1761.

1773. Juan, G. *Mathematician. Hydrography. Measurement of the base-line in South America.*

1775. Dunthorne, *Science and Mechanics. Nautical Astronomy. On the Acceleration of the Moon. Motions of Jupiter's Satellites.*

—— Riccati, *the Geometer of Treviso. On the Integral Calculus, and other Mathematical Treatises.*

1776. Lecchi, *Mathematician. Arithmetica Universalis Isaaci Newton. Geometry and Hydrostatics.*

—— Robertson, *Mathematician. Law of Motion. Conic Sections. Newtonian Philosophy. Elements of Navigation.*

1777. Beraud, Lawrence, *General Philosophy. Theoria Electricitatis. Astronomy and Mathematics.*

—— Zanotti, *Mathematician. Parabolic Orbit for the Comet of* 1739. *History of the Bologna Institute. Miscellaneous Essays on Science and Literature.*

1778. Bassi, Signora Laura, *Professor of Experimental Physics in the University of Bologna.*

1779. Adelburner, *Mathematician. Commercium Literarium ad Astronomia, &c.*

—— Winthrop, of New England. *Transits of Mercury. Comets and Eclipses. Earthquakes. Transit of Venus,* 1761.

1781. Torelli, *Professor of Verona, whose collected Works of Archimedes were printed at Oxford in* 1792. *De Nihilo Geometrico.*

1782. Emerson, *Mathematician. Treatise on Mechanics. Mathematical Researches.*

—— D'Anville, *Eminent in ancient and modern Geography. On the Figure of the Earth.*

—— Hell, Max. *Astronomy, Physics, and Mechanics. Observations on the Transit of Venus,* 1769.

—— Martin, Benjamin, *Optician. On the Newtonian Philosophy. Experimental Physiology. Mathematical Institutions.*

—— Torre, Della, *Professor of Mathematics. Elementa Physica. Microscopical Instruments.*

1783. Bézout, *Mathematician. Theory of Algebraic Equations. Nautical Astronomy.*

1785. Stewart, *Mathematician. Solution of Kepler's Problem. Solar Distance. Various Physical and Scientific Tracts.*

—— Witchell, *Practical Astronomer. On Lunar Observations. The Shadow of a Prolate Spheroid on a Plane. A method of clearing the Lunar Distance.*

1786. Wright (of Durham). *Pannauticon. Claves Cœlestis. Hypothesis of the Universe.*

—— Goodricke, *Practical Astronomer. Observations of Occultations and Eclipses. Various scientific Papers.*

—— Wilson, A., *Astronomer. The Transit of Venus in* 1769. *On Solar Spots. On the adaptation of Cross-wires in Telescopes.*

1788. Voltaire, *of the Scientiæ Amatores. Réponse à toutes les Objections Principales, faites en France contre la Philosophie de Newton.*

—— Whitehurst, *Physicist and Engineer. On the true Length of Pendulums. Philosophical and Mathematical Tracts. Invariable measures of Length, Capacity, and Weight.*

1789. Belgrado, *Practical in Mathematics, Physics, Astronomy, and Geography.*

—— Benvenuto, Carlo, *Promoter of the Newtonian Philosophy. Synopsis Physicæ Generalis.*

1790. Magelhaens, *Physicist. Experiments in Natural Philosophy and Mechanics.*

—— Usher, *Practical Astronomer. Director of the Dublin Observatory. On Eclipses. Improvements in Instruments.*

—— Landen, *Mathematician. On Curvilinear Areas. Summation of Converging Series. The Solar Distance. Calculation of Fluents. Motion of the Equinoxes.*

1790\. Franklin, *Philosopher. Researches on Electricity. On Light and Heat. Meteorology. Animal Magnetism. Improvements in Printing.*

1791\. Fixlmillner, *Astronomer and Mathematician. Meridianus Speculæ Astron. Decennium Astronomicum. Observations for the Tables of Mercury.*

1794\. Wallot, Professor. *Mathematical Investigations. On Chronometric Arcs, &c.*

—— Audiffredi, *Astronomer and Mathematician. Phenomena Celestia Observata, &c.*

—— Du Séjour, Dionis, *Astronomer. Recherches sur la Gnomonique et les Rétrogradations des Planètes. Traité Analytique des Mouvemens apparens des Corps Célestes.*

1795\. Ulloa, Antonio, *Science and Physics. Mensuration of a Degree in Peru. Entretenimientos Physico-Historicos sobre America.*

1796\. Rittenhouse, *Mathematics, Magnetism, Optics, Astronomy. A Collimator. On Timepieces.*

—— Vandermonde, *Practical Mathematician. Elimination of unknown Quantities in Algebra.*

1797\. Titius, of Wittemberg, *Proposer of the empirical Astronomical Scheme, since known as Bode's Law of Planetary Distances.*

1798\. Callet, J. F. *Mathematician. Tables of Logarithms. Telegraphic Language, &c.*

—— Donne, B., *Mathematician. Natural and Experimental Philosophy. British Mariner's Assistant. Mechanical Geometry.*

—— Waring, *Cambridge Professor. On Infinite Series. On Centripetal Forces. Resolution of Attractive Powers. Miscellanea Analytica.*

1799\. Agnesi, Signora Maria, named the Walking Polyglot.* *Propositiones Philosophicæ. Istituzioni Analitiche. Essays in Geometry and Speculative Philosophy.*

1800\. Barrington, Daines, *of the Scientiæ Amatores. An Essay on Horology. Geographical Researches. Natural History. On Climates, &c.*

—— Kastner, *Mathematician. Mechanics and Hydrodynamics. History of Mathematics and the Higher Geometry. Essays on Pure and Practical Science.*

—— Young, M., *Philosopher. Quadrature of Simple Curves. Sounds. Equinoxes. Spherical Errors in Object-glasses.*

1801\. Sylvabelle, *Astronomer. Observations on Jupiter's Satellites. The Transit of Venus in* 1782.

—— Triesnecker, *Various Mathematical Essays. Compiler and Publisher of the Vienna Ephemerides. On Chronological Investigations.*

1802\. Æpinus, *Physicist. Inquiries in Phænomena. Tentamen theoricæ Electricitatis et Magnetisimi.*

—— Darquier, *a practical Astronomer. On the Georgium Sidus. Observations Astronomiques, &c.*

1803\. Jackson, of Exeter, *Musician and Physicist. On Planetary Orbits. Philosophical Letters. Theory of Music. Present State of Music in London.*

1804\. Kant, *Philosopher. Theory of the Heavens. Metaphysical Principles of Natural Philosophy. Mechanical Structure of the Globe.*

—— Ewing, *Mathematician. Author of Institutes of Arithmetic. Practical Astronomy. A practical Discussion of the Doctrine of the Sphere.*

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* This learned and many-tongued lady was Professor of Mathematics in the same University, Bologna, where I afterwards found Professor Mezzofante, the modern Mithridates (See the Cycle, vol. I. page 481-2). Agnesi died a Blue Nun, aged 81 years.

1804. Priestley, *Philosopher. Light and Colours. Discoveries relative to Vision. Chemical Researches. Electricity. Perspective, &c.*

—— Pigott, *Astronomer. On variable Stars. Comets. Transit of Mercury. The Nebula in Comæ Berenicis. Astronomical Observations.*

1805. Aubert, *Versed in portions of Practical Astronomy. Transit of Venus in* 1769. *On obtaining Time by equal Altitudes.*

1807. Bernoulli, John, *Mathematician, Astronomer, and Physicist. Scientific Travels. Lettres Astronomiques. Various useful compilations and translations.*

—— Walker, G. *Mathematician. Doctrine of the Sphere. On Longitude. Treatise on Conic Sections.*

—— King, Edward, *a philosophical Antiquary. Essays on Scientific and Archæological Matters.*

1808. Cousin, *Geometrician. L'Etude de l'Astronomie Physique. Calcul Différentiel et Calcul Intégral. Various scientific disquisitions.*

—— Dalrymple, *Hydrographer. Scientific Essays. On Marine Surveying by the Quin-cunx.*

1809. Barker, Thomas. *Philosophical Researches. On Meteors, Comets, and Mutations of the Stars. Treatise on Meteorology.*

1810. Fleurieu, *Hydrographer. Nautical Astronomy. Voyage to prove certain Time-pieces, &c.*

—— Romanowna, Catherine, Princess of Daschkoff. *Versed in General Literature. President of the Russian Academy of Sciences.*

1812. Malus, *Physicist. Theory of Double Refraction. Reflected Light. Optical Phenomena.*

1814. Cumming, A. *Mathematician and Mechanician. On the Influence of Gravitation considered as a Mechanical Power.*

1815. Nicholson, *Mathematician and Chemist. Essays on and Experiments in Electricity. Differences of Logarithms. Natural Philosophy.*

1816. Schröter, *Astronomer of all Work. Selenography, and the Planets. Memoirs on Astronomical Discoveries. On Lunar Twilight. Constitution of Mercury.*

—— Rios Mendoza, *Mathematician. Tables for Navigation and Nautical Astronomy. Inventor of an improved Reflecting Circle.*

—— Huddart, *Navigator. On Horizontal Refraction. Improvement of Astronomical Instruments.*

1817. De Luc, *Physicist. Atmospherical and Meteorological Researches. On Pyrometry, Hygrometry, Electricity, and Galvanism. On Thermometric Navigation.*

1819. Williamson, *Philosopher. On the Climate of America. Parallax of the Sun. On the Use of Comets. On Thermometric Navigation.*

—— Milner, *Professor of Mathematics. Various Essays on Chemistry, and Natural Philosophy.*

1823. Carnot, *Mathematician and Geometer. Réflexions sur la Métaphysique du Calcul Infinitésimal. De la Corrélation des figures de Géométrie.*

1824. Masères, Baron. *Researches on Negative Quantities. Scriptores Logarithmici, &c.*

—— Dalby. *Mathematical Tracts. The Trigonometrical Canon called General Roy's Theorem.*

1825. Schubert. *On Applied Science. Various Treatises on Astronomy and Mathematics.*

1827. Beaufoy, Colonel. *On the Properties of Air and Fluids. Astronomical and Magnetical Observations. Jupiter's Satellites.*

1830. Rennell, Major, *Geographical Engineer. Observations on Tides. The Currents of the Atlantic Ocean. On the Geography of Herodotus.*

1831. Grégoire, Bishop, *Mathematician. Founder of the French Board of Longitude.*
1836. Lax, *Astronomer. Requisite Tables for Ephemerides. Methods of Clearing the Lunar Distance. On the Divisions of Astronomical Instruments.*
1838. Moll. *Astronomical Papers. On the Adjustment of Weights and Measures, British, French, and Dutch. Spot on Mercury.*
1841. Cacciatore, *Assistant and Successor to Piazzi at Palermo. On Comets. Goniometry. On Refractions. On registering and reducing Astronomical Observations.*
—— Frend, *Mathematician. Theory of Equations. Elements of Algebra. Evening Amusements.*
1842. Innes (of Aberdeen), *Mathematics and Geometry. Calculator of Eclipses, Occultations, and Tides. Various scientific papers.*
—— Ivory, *Geometer. Cubic Equations. Attraction of Spheroids. Refractions. Rectification of the Ellipse. Mathematical Discussions. Cometary Orbits, &c.*
1843. Wallace, *Mathematician. Geometrical Porisms. Quadrature of the Conic Sections. On Fluxions. Mathematical Essays.*
1844. Baily. *Astronomical Catalogues and Tables. Eclipse of Thales. Repeater of the Cavendish Experiment. Phenomena in Eclipses, &c.*
—— Dalton, *Mathematician and Chemist. Meteorological Observations and Essays. Atomic Theory. Colours. Density of Water.*
—— Henderson, *Astronomer. On Comets. Moon-culminating Stars. Computer of Occultations. Detector of the Parallax of α Centauri.*
1845. Cassini IV. Count, *Mathematician. The last of the Cassinis. He died aged* 97.
—— Damoiseau, Baron, *Geometer. Elements of Halley's Comet. Researches on Satellites of Jupiter. Lunar Tables from theory alone.*
—— Bessel, *Astronomer. Precession of the Equinoxes. Planetary Perturbations. Astronomical Observations, Researches, and Reductions. Discovered the Parallax of* 61 *Cygni.*
1847. Pearson. *On Clocks, Time-pieces, and Orreries. Catalogue of Zodiacal Stars. On Astronomical Instruments. Treatise on Practical Astronomy.*
—— Hodgson, General, *Surveyor. On Indian Geodesy. Heights of the Himalaya Mountains, &c.*
1848. Herschel, Caroline. *Astronomical Observations. Sidereal Catalogues. Comets.* Died aged 97.
—— Taylor, *Astronomer. Re-examination of Piazzi's Palermo Catalogue at Madras. Experiments on Invariable Pendulums.*
—— De Vico, *Astronomer. Spots on Venus. The Rings of Saturn. Discoverer of six Comets.*
—— Goldingham, *Astronomer. Observations made at the Madras Observatory. Scientific Papers.*
1849. Trechsel. *The Berne Observations. Swiss Triangulation. On Geodesical Operations.*
—— Wilcox, Colonel, *Director of the Lucknow Observatory. On Trigonometrical Reductions, &c.*
1850. Schumacher, *Astronomer. Planetary Ephemerides. Jahrbuchs. Established the Astronomische Nachrichten, &c.*
—— Galbraith. *Mathematical and Geodesical Essays. Astronomical and Meteorological Tables.*
—— Cerquero, *Spanish Naval Officer. Improvements in Nautical Astronomy. Director of Cadiz Observatory. Proposer of a new artificial Horizon.*
—— Beer, *Mathematician and Astronomer. Many years mapping the Moon with Dr. Mädler. Astronomical Communications.*

1851. Oersted, *Physicist. Laws of Motion. Physical Investigations and Experiments. Discovery of Electro-Magnetism.*

—— Boguslawski. *Various Treatises on Practical Astronomy. A difference-micrometer. Inquiries respecting Falling Stars.*

—— Galloway, *Mathematician. On Trigonometrical Surveys. On Meteorites, or Bolides. Researches on the Sun's Proper Motion by means of Southern Stars.*

—— Goldschmidt, *Mathematician. Finite and Infinite Analysis. Magnetism. Calculus of Probabilities. Various philosophical papers.*

1852. Colby, General, *Director of the Trigonometrical Survey of the United Kingdom. Scientific Suggestions in Geodetical Operations.*

—— Lockhart, *Mathematician. Various scientific Essays. Numerical Solution of Equations.*

—— Dollond, George, *Optician and Mechanician. Proposer of the spherical crystal Micrometer, and Macro-micro-lens. Self-registering Meteorological Frame.*

1853. Arago, *Astronomer, Mathematician, and Physicist. Popular Astronomy. Various Papers on Magnetism, Meteorology, Biography, &c.*

—— Walker, Sears, *Mathematician. Versed in Astronomy, Mathematics, Geology, and Languages.*

—— Lavigne, *Geometrician. Wrote various Essays on Optics and general Science, and on some branches of Mathematics.*

1854. Lindenau, Baron, *Director of the Seeburg Observatory. Monalitche Correspondenz. Essays.*

—— Petersen, *Assistant and Successor to Schumacher in the Observatory at Altona.*

—— Riddle, *Mathematician. Navigation and Nautical Astronomy. The Double-altitude Problem.*

—— Mauvais. *Mathematics and practical Astronomy. On Comets. Scientific Essays.*

1855. Gauss, *Physicist. Disquisitiones Arithmeticæ. Theoria Motus. Experiments in Magnetism. Various Geometrical Theorems.*

—— Sheepshanks, *Astronomer. Natural Standards of Length and Weight. Chronometric Arcs of Meridian. On Astronomical Instruments.*

1856. Goujon, *Geometrician. Mathematics applied to Machinery and Astronomy.*

—— Biela. *General Mathematics. Discoverer of the short-period Comet bearing his name.*

—— Beechey, Admiral, *Geographer and Arctic Voyager. Voyage round the World. On the Tides in the British Channel and Severn River.*

1857. Colla. *Practical Astronomy and Meteorology. Obtained the King of Denmark's gold medal for discovering a Comet.*

—— Beaufort, Admiral. *Versed in all branches of Hydrography and Nautical Astronomy. Survey and Description of Karamania.*

—— Cauchy. *Mathematician and Geometer. Polyhedrons. Fermat's Theorem. Propagation of Waves. Planetary Motions. Versed in pure and practical Mathematics.*

—— Owen, Admiral W. F., *Hydrographer. Extensive surveys of the Coasts of Africa, Lakes of Canada, and Bay of Fundy.*

1858. Peacock, George, Dean of Ely, *Mathematician and Astronomer. Treatise on Algebra. Standards of Weight and Measure. Life of Dr. Thomas Young. History of Arithmetic, &c.*

From this register, we turn to the second chapter of the Cycle—

A GLIMPSE OF THE SOLAR SYSTEM.

THE SOLAR SPOTS (Cycle, I. page 86-90). Since my account of these remarkable phenomena was published, much additional matter has come to light respecting the surface of the mighty centre of our system; for which we are mostly indebted to the labours of Herschel, Secchi, Schwabe, Dawes, Schmidt, and Barral. It seems to me that the discovery of solar spots in 1611, recently so contested, should be assigned to the astronomer who first applied optical power to them; for assuredly they had been so often seen with the naked eye, as to have become proverbial. When citing Kepler on this topic at page 88, I ought to have mentioned his summoning Virgil as a witness (Geor. i. 441):

"Ille ubi nascentem maculis variaverit ortum."

And to controvert those who only recognise clouds in spots on the sun's face, the poet adds another significant line (Geor. i. 454):

"Sin maculæ incipient rutolo immiscerier igni."

In the second part of the history of the renowned Knight of La Mancha, the humorous Sampson Carrasco comforts Don Quixote as to the blemishes which critics decried in his first narrative, inasmuch as they could only be compared to little spots on the bright sun—"*sin alcernerse à los atomos del Sol clarissimo de la orba de que murmuran.*" Now as this book was licensed for publication in 1615, Cervantes must have written it still earlier; and in all probability without having heard of the discussions on Galileo, Fabricius, and Scheiner. Captain Stanyan, who first recorded the "flaming prominences" revealed during total solar eclipses (Cycle, I. page 133), by him called *blood-red streaks of light*—observed the solar spots with his "18-foot glass" in the years 1703 and 4, apparently acting by the advice and "according to the rules received from Mr. Flamsteed." In this report, which is printed in the xxivth volume of the Philosophical Transactions, pages 1756 to 1762, he mentions his having seen the "cloud or misty matter" (*penumbra*) which surrounded the spots. About a dozen years after, solar maculæ may have

become visible to the unaided eye, so as to form a topic of conversation, whence Prior's quidnuncs of Down Hall are thus alluded to:—

"But what did they talk of from morning to noon?
Why of spots on the Sun, and the man in the Moon."

Yet although maculæ may long have been seen, their periodicity watched for, and their obvious fitness for marking solar rotation made use of, it is only very recently that their startling connection with terrestrial magnetism became suspected; and as a wonderful coincidence seems to be satisfactorily established therein, a mine is sprung, the extent of which he is a bold man who will venture to predict, although inductive experience is allowed to adopt the tone of prophecy. Sun-painting, already under such wide and rapid improvement, to which we shall return in the sequel, is one of the marvels of the age; but what may we not yet have to say, and that ere long, on the analogies binding earth with the orb of day?

To proceed. In the important volume of the results of Sir John Herschel's celebrated visit to the Cape of Good Hope, he gives a whole chapter (vii.) to observations of the solar spots; with figures which were delineated from magnified images formed by means of a 7-foot achromatic refractor upon paper pinned on a wooden screen. One of these spots, seen on the 29th of March, 1837, was a monster indeed, since it occupied an area of nearly five square minutes:* "now a minute in linear dimensions on the sun being 27,500 miles, and a square minute 756,000,000, we have here an area of 3,780,000,000 square miles included in one vast region of disturbance, and this requires to be increased for the effect of foreshortening." Of another spot, not the tenth part of the preceding one, he proceeds to say—"The black centre of the spot of May 25th would have allowed the globe of our earth to drop through it, leaving a thousand miles clear of contact on all sides of that tremendous gulf." This is absolutely

* I considered that a very compact specimen had been selected, when on the 6th of June, 1828, I measured, with a double-wire micrometer, a remarkably black spot, which, without its umbra, was 13,000 miles in diameter; as communicated to the United Service Journal for 1829, part I. page 534.

astonishing, and we are perplexed between the Herschel hurricanes and cycles, and the Carrington currents; but the theory of the formation of these areas can hardly be expected to approach certainty while the facts are so few in number, and so limited in character. Nevertheless, the practical reasoning of the following passage from the same author, demands the fullest attention:—

"We are naturally led to inquire for an efficient cause—for a *vis motrix*, to give rise to such enormous dynamical phenomena, for such they undoubtedly are. The efficient cause of fluctuations in our atmosphere, in terrestrial meteorology, is apparent enough, namely, external agency—the heating power of the sun. Without this, all would be tranquil enough. But in the solar meteorology we have no such extraneous sources of alternate elevations and depressions of temperature altering the specific gravity and disturbing the equilibrium of its atmospheric strata. The cause of such movements as we observe, and upon so immense a scale, must therefore reside within the sun itself, and it is there we must seek it."

By continued and close observations of the solar spots for upwards of a quarter of a century, M. Schwabe, of Dessau, has proved their periodicity, and that their number varies considerably at different epochs; he has also given a series of the years in which he found them more or less plentiful, showing that although the maxima and minima are not conclusively settled, yet in the observed increase and decrease of the groups, there is sufficient regularity to countenance the theory—hitherto so vague—of their agency on the weather; and the following are his results:—

Year.	Groups.	Year.	Groups.	Year.	Groups.
1826	118	1837	333	1848	330
27	161	38	282	49	238
28	225	39	162	50	186
29	199	40	152	51	151
30	190	41	102	52	125
31	149	42	58	53	117
32	84	43	34	54	67
33	33	44	52	55	38
34	51	45	114	56	34
35	173	46	157	57	98
36	272	47	257	58	188

What a lesson is here afforded to the working Scientiæ Amatores! Until lately there were many of that corps who like myself were in the habit of gazing

at the black maculæ, the torchlike faculæ, the light-emitting luculi, the penumbræ, nuclei, radii, and what not, sometimes drawing, and at others measuring, those objects; but, from not adopting a systematic siege, it was mostly labour thrown away upon what Delambre branded thus, "*Il est vrai qu'elles sont plus curieuses que vraiment utiles.*" Still a conviction had obtained that in constitution the centre of the sun was a dark, and comparatively cool, body, encompassed by a thick photosphere, or external coating, with an agitated incandescent surface; nevertheless the mine requires working, and amateurs are especially called to the task. I alluded to a method of watching the sun at page 87 of the Cycle, and since then Sir John Herschel has proposed a very effective helioscope, see his Cape Observations, pp. 436-7. Mr. Dawes has also introduced his admirable solar eye-piece, which possesses the advantage of being applicable to any existing telescope.* The solar phenomena are therefore more within our ken than formerly as to their shape and habits, or successive changes, and partially as to their effects; but their physical constitution remains unfathomable, like the question, whence come the perpetual beams of light and heat of the orb itself? Yet these particulars, the theme of endless plausible cogitations and ingenious suggestions without proof, though still among the unrevealed mysteries of nature, may finally submit to unremitting researches.

For instance, the solar spots can now be safely pronounced to be no longer an object of idle curiosity, like the casual clouds of our atmosphere. The few landmarks hitherto recorded begin to indicate a regular progress of position in them, with periodical maxima and minima in their amount. Thus in the years 1845-46 the groups of solar spots extended to about 40° north of the equator, and to 30° south of it, leaving a central blank band from 8° north to 5° south. This state

* The following details from my indefatigable friend's own pen may be acceptable: "In my paper to the R.A.S. in 1852, and published in their Memoirs the next year, I described a *solar eye-piece* invented by myself, and made under my directions by Dollond. I have constantly employed it on my refractors of 7½ and 8 inch aperture, and even on Lassell's 24-inch Newtonian with the whole aperture, yet never broke a dark glass even in fine weather, and under a power of 500. In January '52 I observed a spot rotate through an arc of 100° in six days, which was the first announcement. Far from Secchi's being the first, his observation was made with one of my solar eye-pieces; and in the Astron. Nach. he acknowledged that he estimated the heat of various portions of the sun's disc at my suggestion made in the Monthly Notices of 1851."

of arrangement is now recurring, with the exception that the preponderance is at present on the south side of the equator. In 1853 and 4 the spots were distributed from 20° north to about 20° south, decreasing in number till early in the year 1856, when there was a decided minimum, and the equatorial region remained clear; but spots appeared in both hemispheres from 20° to 40°. Their parallels are already again contracting.

So much for their *position*, and now for the *motion*. Their daily drift in longitude reveals a general equatorial current 30° in breadth in the direction of the rotation, and a reverse current of nearly the same breadth is perceptible beyond it in each hemisphere.

In the foregoing details we are greatly indebted to Mr. Carrington of Redhill; but now we must turn to Major-General Sabine, with thanks for the light that he has shed on the mysterious correspondence between the solar and the terrestrial magnetic disturbances; for a decennial period is therein indicated, as well as another connected with the earth's orbit, which brings us to the present wondrous result of well-directed investigation.

No branch of science has sprung forward with more true bearings in theory and experiment than terrestrial magnetism, since the impulse given to it by my valued and venerable friend, Baron von Humboldt, in 1836, followed up by the skill of a battalion of zealous inquirers led on by Gauss, Hansteen, Weber, Sabine, Erman, Arago, Kupffer, and others. The Germans found the disturbances and consequent fluctuations of their *declination*-magnets contemporaneous in all their stations, yet the deflections at some other distant observatories differed *inter se*, and from the European, even after due allowance had been made for the local antagonistic force of the earth, although they agreed in time.

The British Colonial establishments in both hemispheres were therefore directed to investigate the laws of the magnetic *declination*, *inclination*, and *intensity*, carefully eliminating the effects of occasional disturbances. Then, to discover the unknown causes of disagreement, it was determined first to analyse the experiences of single stations separately, and follow up any symptom of periodicity or laws in the phenomena. We are not now about to treat of the

isoclinal, isogonal, and isodynamic lines of our magnetic system, or its mysterious perturbations, albeit I am gratified at seeing the harvest which is resulting from measures emanating from the line of conduct recommended by the Council to which I belonged; suffice it here to allude to the recent revelation of the prevalence of a principle which may yet prove, though not so invariable, yet equally coextensive with gravitation.

The unvarying atom (if we may borrow the term from chemistry) is the mean solar day as to time, but the increase of each of the three magnetic elements is subject to a different code of laws to what the decrease is, and these six codes moreover are found to differ at each station. The hourly deflection in declination through the solar day, may be simplified by remarking, that this effect is much weaker in the night than in the day. It includes several minor variations, yet all influenced by the hour: thus in the northern hemisphere, the utmost easterly deflection occurs early in the forenoon, and the utmost westerly in the afternoon; whilst nearly the contrary obtains in the southern. Then as to amount, it is zero at the magnetic equator, increasing towards the middle latitudes, both north and south. An additional dependence on the sun's position is revealed by an increase of the above deflection at the summer solstice on our side of the equator; and at the winter solstice on the south side. And, what is still more in the argument, the magnetic equatorial line varies daily except at the equinoxes.

The daily dependence of magnetic intensity on the solar altitude had long been known and recorded; but about eight years ago, M. Lamont, of Munich, announced that this patent variation was liable to another which indicated a period of ten years: and this was shortly after fully confirmed by General Sabine, from a rigorous discussion of the observations made at the far distant magnetic stations of Toronto and Hobarton. In the course of these operations, the General detected another important concomitancy which had escaped Lamont, namely, that the periods of maximum variation in the needle correspond with those of the maximum abundance of solar spots, and *vice versâ*. Hardly had this astonishing announcement been made, when intelligence arrived that Pro-

fessor Gautier of Geneva, and Professor Wolff of Berne, had come to the same decision, and independently of each other, from a perusal of Lamont's inferences. This recognition of the coincidences in solar and magnetic periods was very exciting, as a proof of their cosmical affinity, and still more so on considering the conclusion arrived at by my persevering friend Sabine, namely, that "in both our hemispheres, the period when the intensity of the earth's magnetic force is greatest, and the needle most nearly vertical, is from October to February, which is just the season when the earth is nearest to the sun, and is moving with the greatest velocity in its orbit." Hence when Mr. Carrington sent me a graduation of the respective solar and magnetic curves in decennial periods, clearly showing the reciprocity of emotion between the sun and earth, I could not but contrast their importance with the boasted curves—*visum effugientes*—of Apelles and Protogenes.

But although this wonderful result was so contemporaneously enunciated, General Sabine has distinctly shewn the relative priority of the three by his Paper read to the Royal Society in March 1852, and published in the Philosophical Transactions: he had, moreover, told me of it previously. He is following the matter up with avidity, and lately wrote to me thus:

"I suppose that you have got your Phil. Trans. for 1856. In article xv. you will find another paper from me in continuation of that of March 1852; and in article i. of 1857—a copy of which you shall have, if the printer be a true man, early next week—you will find a third, shewing by the analysis of eight years of hourly declination*—observations at Hobarton, the existence of the decennial period in the solar magnetic variations, and its non-existence in the lunar magnetic variations.

"The good old man at Berlin (*Humboldt*) was amongst the very first to recognise the importance of the connection between the magnetic variations and the solar spots; and he has taken the pains, in a note to page 200 of the IVth volume of Cosmos, to state very positively the priority of my paper over the publications of Wolf and Gautier. Wolf has recently announced his discovery of an annual period of the solar spots, which he says accords with my deduction of an annual period in the magnetic disturbances. This puzzles me greatly. I can quite well understand that the magnetic

* As a sailor, I again object to this use of a word so immemorially used as belonging to the sun. It is true the needle may *decline*, but nomenclators ought to have introduced a term for this variation without poaching on the practice of the navigator. DECLINATION is in sound use among Seamen.

disturbances may be due and dependent on the solar spots in some way; and that the affections of the earth, though due primarily to the sun, may have a subordinate affection produced by the earth's annual revolution round the sun. But how the earth's revolution can influence the solar spots is a puzzle: so, as yet, is the mode in which the lunar magnetic variation is produced by the moon. But the greatest puzzle of all is, the terrestrial secular magnetic change, knowing, as we now do, that a precise aliquot portion of the annual amount of change takes place in every week of the year—a change as regular as your observatory clock, and yet having no traceable connection with any other phenomena either terrestrial or cosmical."

Now if it should prove that this unlooked for coincidence of the decennial laws of frequency with the solar spots and the magnetic disturbance are not accidental, we may next ask whether the sun does not act as a magnet, or electro-magnet, and whether the currents thereby developed are within its atmosphere? If so, my friend Allan Broun's scene opens,—as the said spots may be disruptions of that current, due to the planetary positions with reference to the solar equator! Knowledge, however, has not yet advanced sufficiently to yield correct ideas of the effects of magnetic action, gases, volatilizing materials, pressure, and other physical liabilities on or beneath the sun's surface. But this is treading on hot ashes; for the very admission of magnetism and electricity as agents in our sidereal system, will alarm the *moderados* in cosmogony, and philosophists in general.

On Solar Eclipses (Cycle, I. page 138 to 143). The advantages offered to physical inquiry by total solar eclipses, has been strongly manifested since the publication of my remarks; and especially by that of July 1851, which was systematically observed by English, French, German, and Russian astronomers. The several phenomena were duly watched, and the beautiful luminous ring, called the Corona, from its radiating striæ, reminded the Astronomer-Royal of the ornament usually engraved round a mariner's compass-card. Nor can there be now any reasonable doubt of its pertaining to the sun itself, or that it is rendered visible by the interposition of the moon.

With reference to the red prominences cited at page 133 of the Cycle, there are traces of them also in the observations of Short, Ulloa, Ferrer, and Van Swinden. In 1706, Flamsteed attributed them to the moon's having an at-

mosphere; and the mind of B. Vassenius received a similar impression while he was observing the eclipse of the sun in 1733:—

"At the time when the entire sun was covered," says the Swedish astronomer, "I saw, besides the greater part of the markings in the disc, the atmosphere of the moon, through a telescope of 21 feet; and observed it on the western limb of the moon, at the period of greatest immersion, a little brighter; and yet without the irregularity and inequality of the luminous rays, which affected the eyes of those who observed it without a telescope. Highly worthy, not only of the admiration but also of the judiciary opinion of the most illustrious Royal Society, appeared to be certain reddish spots in it *(subrubicundæ nonnullæ maculæ in illa—that is, in the lunar atmosphere)*, three or four in number, which were seen outside the lunar disc; one among which was larger than the rest, occupying about the middle space between the south and the west, as near as one could judge." (Philosophical Transactions, vol. xxxviii. page 135.)

Hence it is evident that Vassenius was of opinion, that the anomalous light which he perceived was actually the atmosphere of the moon. The eclipse of 1842 was useful in reviving this subject, although the observers, having forgotten or overlooked existing records, were taken by surprize, thus proving Bacon's remark on discoveries which may result from not reading. But this did not prevent a recognition of the physical fact of protuberances around the solar periphery, though the being thus taken aback was the cause of the gazers having neglected instrumental means to probe the nature of those strange appearances. Beautiful drawings however were made, the fact was fully confirmed, and the prominences raised in importance as part and parcel of the solar orb. The Astronomer-Royal seems to have been the first to dismiss the notion of their being lunar projections; assuring me that he considered them to belong to the sun, and, if so, they must be mountains, or masses of flame, upwards of twenty or thirty thousand miles high.

At length the great eclipse of 1851 approached, when a numerous band of qualified observers repaired to selected posts. The mystic protuberances had been till then chiefly believed to be conical in shape, but they now appeared in somewhat different forms to different people: thus Airy describes one as resembling an Australian *boomerang;* Mr. Dawes compares it to a Turkish scimetar; Mr. Carrington calls it a curve or bow of light; and several thought it like a sickle: the drawings therefore in the Monthly Notices of the Royal Astro-

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nomical Society open a singular field. The Council of that Society consider it to be decisively proved, that these phenomena belong to the sun, because the protuberances "that were observed on the eastern limb became quickly hidden, while others sprang up on the western limb; that is, they were respectively covered and revealed on the eastern and western limbs of the sun by the advancing moon." This, however plausible, is not conclusive; the case being still beset with various difficulties, for much of the argument is rather ingenious assumption than proof. Being thus in a "fix" in this matter, I last year wrote to Mr. Airy, begging to know whether any alteration had taken place in his opinion respecting these evanescent lights, and, in his obligingly prompt reply, he said, "I have nothing more to say about the red flames, believing (as formerly) that they ooze out of the sun, but puzzled (as formerly) by their different qualities to different observers." It is to be hoped that this question will receive its quietus in 1860; but many years may elapse before all the conditions of such vast appendages to the grand luminary shall be satisfactorily known. May they not indicate the existence of an extensive solar atmosphere?

Another chance for physical research dawned upon us this last year in the interesting eclipse of March the 15th, and, as it was to be annular—one of the most beautiful of the celestial phenomena—I was for a time undecided whether to repair to the line of annularity to watch physical details, or remain at the Observatory and catch what I could with my old equatorial. Having passed the age of three score and ten, and no other eclipse of this class being expected till August 1887, I was greatly relieved by a kind offer from Mr. Isaac Fletcher to come from Cumberland, and aid me in any way I might suggest. A synod being called, it was resolved that Dr. Lee and Mr. Fletcher should proceed to Towcester, in Northamptonshire, armed at all points for noting every change—especially that curious effect of irradiation, the formation and dispersion of the so-called Baily's beads;* while I was to remain stationary

* This alliterative epithet has been recently adopted; but it does not imply that they were first seen by Mr. Baily in 1836; they having been described or indicated by different observers, as Halley, Maclaurin, and Short.

and measure the passages of the cusps, taking every advantage of fixed instruments. This is what followed:—

"The 15th opened with as fine a morning as possible, the sky being clear, with a light air of wind, and I therefore hastened to Hartwell in order to map the spots on the sun; but before 10 A.M. the welkin was shrouded. However I prepared the batteries for attack, shipped the micrometer with a positive eye-piece of 62 power, reduced the aperture of the object glass, threw open the observatory to assimilate the temperature, and saw all the meteorological instruments in their proper places. The wind now freshened up from the N.W. and there fell a little rain at intervals: but at about $11^h\ 35^m$—as if to shew that all was right beyond the clouds—the latter became fleecy, so as to reveal the luminary, although a driving scud was passing over its surface. The telescope was now carefully pointed to the disc-place, where the moon would approach the sun in the inverted field; and while catching a firmish gaze, *a startling tremor of the limb*, followed by a faint dark line, announced the commencement of the eclipse's first contact, at $23^h\ 08^m\ 25^s$ sidereal time. As this line strengthened, masses of cumuli again got the mastery; but shortly after noon the sky partially cleared, and the moon was distinctly seen nearing a remarkable group of spots beyond the centre, in the *n f* quadrant, the closest outlier of which she occulted at $23^h\ 49^m\ 56^s$ sidereal time. The cusps were then very acute, and the lunar limb was sharp and smooth: but immediately the clouds again clustered, and the sky continued densely covered with cirro-stratus till nearly sunset.

"During this time external observations were not neglected, but nothing remarkable was seen; which is not surprising when the nature of the phenomenon, the season of the year, and the character of the day, are recollected. Towards the time of central eclipse the atmosphere became of a leaden-violet tinge, chilly and gloomy, resembling the coming on of a squall or thunder-storm; yet never too dark for my reading the hour-circle of the equatorial without artificial light; though a candle was required in the transit-room for reading off the attached thermometer. It seems that 992 parts of 1,000 were to be covered at Hartwell; and even on the central lines the un-eclipsed portion of the disc was computed at 13345″, which would suffice to constitute a luminary 1′ 10½″ in diameter. At the greatest darkness there was still a lurid whiteness on near objects; the peafowl were strutting about unconcerned and feeding, but the finches and sparrows ceased chirping, and were evidently uneasy, while the rooks came clamorously cawing from all parts to their nests on the trees to the south-east of the observatory. After the deepest gloom the light increased rapidly, and as it brightened a cock crowed, and the rooks winged off again.* Of the vegetable

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In my Cycle (vol. I. p. 480) is Baron von Zach's impression when that zealous astronomer and I travelled to Bologna together to observe the annular eclipse of September 1820: "Avant que l'attouchement parfait des deux bords fût effectué, on voyait, non pas un filet continu de lumière, mais des petits points lumineux, comme autant de grains brillans dans une file de perles, séparés par des interstices obscurs." As the miserable Queen of France's diamond necklace had just been publickly talked of again, we joked on its possible exaltation, like Berenice's hair. Still, it may be well to warn the tyro, that the 'beads' are of no physical import.

* A question may here be raised between the astronomer and the naturalist, as it might be averred that the

world there was little to remark upon, as pimpernels and the like were not yet in flower; but the crocus exhibited emotion, and a delicate Cape acacia in my house (raised from seed sent by Mr. Maclear) folded its leaflets as it does at night-fall, after the manner of one of its predecessors in the eclipse of 1836, as mentioned in the Cycle, vol. I. p. 143.

"Meanwhile the secondary details were closely attended to; and as it may aid in estimating the effect produced on the temperature of the atmosphere by the loss of so large a portion of its usual light and heat—even with the wet blanket of clouds between—I subjoin the register as noted by Mrs. Smyth at Hartwell:—

TIME.	Exposed Therm.	In the Observatory.		
		Barom.	Therm.	Hygrom.
H. M. At 9·30 A.M.	50 4	29·67	43·8	·489
11·30 „	51·3	29·69	45·0	·470
12·30 P.M.	50·5	29·70	44·8	·437
1·00 „	47·8	29·71	44·0	·456
2·15 „	50·4	59·72	46·6	·475
3·00 „	51·4	29 70	47·9	·499

While this was occurring at Hartwell, my annularity-colleagues had arrived at Towcester, and taken such promising stations as at any rate to deserve success; but there the phenomenon was almost a failure, except in *ré* Baily's Beads, as will be seen by the annexed communications. On returning home, Dr. Lee reported himself as follows:—

"March 15. It was a fine clear morning, but already at 9 A.M. the sky became hazy, clouds began to form, and before ten the sun disappeared. This state of weather, with occasional light rain, continued till the eclipse was approaching its central phase at 12^h 50^m, when a very visible dulness ensued, but might have been attributed only to the clouds and rain, had we not known that

rooks were merely rushing home for the vernal purpose of building their nests. Many might thus be employed, but the trees abound also with old nests, and I saw the birds arrive in swarms during the eclipse, and depart again after it. The following morning I met Mr. Todd, of the Sedrup Farm, who told me that as the darkness came on he noticed the rooks flying over his grounds towards Hartwell; adding that his poultry evinced an inclination to roost but did not. Two of Dr. Lee's people, George Carter and Joseph Horton, also noticed those birds coming from various quarters to their accustomed trees, with the advance of the eclipse.

there was an eclipse *in transitu*. Up to this time I had been stationed at a handy telescope, which, with an excellent pocket chronometer, had been lent me by Mr. Dollond, the optician; they were steadily placed on the uppermost of two casks. Mr. S. Horton was at my side recording the variations of a maximum and a minimum thermometer, supplied by Messrs. Negretti and Zambra, arranged on a temporary stand. Mr. Watkins was watching a barometer close to us, and Mr. Fletcher with your $3\frac{1}{2}$ foot achromatic was at his post by a cucumber-frame at a little distance. Mr. Huggins was near us with his photographic apparatus, and on his right was Mr. Bousfield, all ready for action, but unfortunately no sun appeared.

"The church tower, said to be 92 feet high, was to the south of us, and we perceived some gazers on its summit, whereupon Messrs. Fletcher and Huggins decided on ascending it, with our worthy host for their guide. At about 1 P.M. the atmosphere was certainly darker and duller than it had hitherto been; but still, on directing the telescope to the tower-division, their countenances were distinguishable. Suddenly the sun appeared through the clouds for half a minute, in which brief interval we caught a sight of the eclipsed portion of the solar disc, and a general cheer arose. It was becoming still darker, and an evening gloom came on, with an effect like a cold chilly London fog. In a few minutes, although the sun continued to be invisible, this strange feeling diminished, and at $1^h\ 15^m$ it was certainly lighter, but with small rain. At $1^h\ 25^m$ we again caught a glimpse of the progress of the eclipse for a few seconds, and then, though the rain had ceased, and the weather had cleared off, masses of cloud hid the sun until some time after the eclipse had terminated.

"During the greatest darkness I observed that the trees in the park, at the bottom of the garden, became confused for five or ten minutes, as seen at night. All observation of other physical peculiarities succumbed to weather, of the state of which, from 11 A.M. to 2,30 P.M. we made a careful record."

Mr. Fletcher very promptly related his proceedings to me thus :—

"On Monday morning we made every preparation for observing the eclipse, although the sky became gradually overcast with clouds of rapidly increasing density. The site we selected was the open garden attached to the residence of Mr. Watkins, who showed us every possible attention. Already, between 10 and 11 o'clock, the sun's place could not be discerned at all, and at $11^h\ 30^m$ we feared that we should only be able to note the general effect of the eclipse on the surrounding landscape; for this purpose I repaired to the summit of the tower of St. Lawrence's church, which commands an extensive view in every direction. Thinking it might be difficult to get your telescope up the spiral staircase of the tower without risk of accident, I left it in the garden for the use of Mr. Bousfield, and took with me his smaller instrument, a capital 30-inch achromatic of $2\frac{1}{4}$ inches aperture, by Ross. I pointed it to the supposed place of the sun, and remained in watchful readiness to observe, should opportunity occur. At $12^h\ 53^m$ (approximate Greenwich mean time) there was a perceptible increase in the gloom, which gradually shrouded the landscape, but which was not greater than might fairly be attributed to so dense a canopy of cloud without resorting to the eclipse for an explanation. At 12,55 the gloom deepened, and surrounding objects assumed a peculiar tinge,—a dingy, yellowish neutral tint, chiefly prevailing towards the east.

"Up to this time neither the cattle or sheep in the adjoining fields, or the starlings populating the tower, indicated any thing peculiar. At 12,58, or thereabouts, a loud cheer from the persons on the tower and in the streets below, announced that the eclipsed sun had presented itself for a moment. At first there was an exquisitely fine crescent with long delicate cusps, almost surrounding the moon, but vapour of variable density, at a greater elevation than the clouds to which I before alluded, drifted with great velocity across the lunar disc, and rendered it invisible for perhaps a minute. During an instant only the phenomenon revealed itself, and I saw the rupture of the annulus, for on the north following limb of the moon I caught an evanescent glimpse of five or six little egg-shaped beads, one rather longer than the rest. In less than a second all was permanently buried in a sea of cloud until too late. At $1^{h}\,3^{m}$ the gloom had sensibly decreased, and in a few minutes afterwards it was scarcely noticeable. The maximum gloom even was never great. Hanslop spire, eight miles to the eastward, continued visible all the time, and the nearer objects were obvious enough, though dim. While the comparative darkness was at its maximum, the starlings flew about, as if scared, but the cows were lying down, and the sheep continued to graze unconcerned. I may add that in the morning I had seen a large group of solar spots near the centre of the disc, and that they afterwards became visible without instrumental power, by merely using the precaution of a coloured glass.* I measured this group next morning with the wire micrometer of the Hartwell equatorial, and found it to be 114,000 miles long, and 65,000 miles broad."

Still, in hopes of making a whole out of disjointed observations, I inquired of several friends for their "experiences" of the phenomenon. In reply, Professor J. C. Adams wrote to me from Cambridge on the 24th of March:—

"With respect to the comparatively prosaic eclipse of last week, I was very unfortunate. I went with Challis and another friend to Peakirk, near Peterborough, in order to be on the central line; but we hardly caught a glimpse of the sun the whole time. They were much more fortunate at our Observatory here, having procured some pretty good observations."

And Mr. George Biddell Airy, the Astronomer-Royal, to whom I communicated this intelligence, remarked, by return of post:—

"However badly Adams saw the eclipse, we at Wellingborough saw it worse, or rather did not see it at all. From the time that our telescopes were planted, say three quarters of an hour before the eclipse began, we only saw the sun, not the moon, about two minutes, in which we focalized the eye-pieces. During the eclipse, the sun and moon had *not once* a limb, even the rudest, in a telescope. My party at Bedford got a few sextant measures, my party at Market Harborough got one sextant measure, and my people at Greenwich next to nothing."

Mr. Thomas Dell, of Aylesbury, went to Peterborough to be in the line of annularity, and wrote to me from thence as follows:—

"The weather during the eclipse on Monday last was very unfavourable; from a minute and

* This was also seen by me and by my family under similar circumstances. (W. H. S.)

a half after the commencement, till $12^h\ 40^m$ (Greenwich mean time), the sun was entirely obscured by dense clouds, but from that time up to $1^h\ 3^m$ it became almost continuously visible through a light haze. My site was to the eastward of the cathedral, on the north bank which crosses the Bedford Level, so that I had a good view of the surrounding country. The gloom, up to $1^h\ 0^m\ 50^s$ was not greater than frequently precedes a thunder storm, but then increased rapidly, causing the feeling, as described by Captain Biddulph in the total eclipse of 1851, 'of something material sweeping over at a frightful speed.' The clouds near the sun appeared to descend rapidly, and, losing their bright borders, became intensely dark. At $1^h\ 1^m\ 4^s$ the light suddenly increased again, more rapidly than it had previously decreased, so that, in 5 or 6 seconds more, the deep gloom had disappeared. A cloud passed over the sun at the very moment that the annulus was about to be formed, and its duration was certainly not more than 2 seconds, therefore the time of the formation would be $1^h\ 1^m\ 2^s$. This shews the accuracy of the line laid down on the Ordnance Index Map, for, as my position was about 5½ miles S.E. of that line, it is probable that I was close to the limit of the shadow."

So much for the annular eclipse of March, 1858.

Solar Translation (Cycle, I. pages 281 to 284). The hypothesis of the whole solar system journeying through space towards a point somewhat to the north of λ Herculis, has received intense attention since my Cycle was published. Even then I thought that, from the rigid accuracy of modern observations, the doctrine of a central body would either be positively accepted ere long, or rejected without further hesitation; for, though the idea that our sun, attended by his numerous *cortège* of primary and secondary bodies, together with all the comets of our complexure, was in progress around some incomprehensibly distant and enormous orb, was astounding, the inferences of science were too strong to admit of reasonable doubt. My lamented friend Thomas Galloway, a valuable member of each of the scientific bodies to which he belonged, obtained—at my instance—the royal medal of the Royal Society by his deep researches on this topic. The results of his movement of the solar system, derived from the proper motions of the southern stars, were contained in an able memoir inserted in the Philosophical Transactions for 1847; and the conclusions of this able mathematician have been further established by the elaborate calculations of the Rev. Robert Main. This gentleman, in his essay on the proper motions of the stars, contained in "the Greenwich Twelve-year Catalogue" (*read in March*, 1850), has formed a table (III) by extracting those

stars in his general table which have proper motions greater than $0^s{\cdot}02$ in Æ, and greater than or equal to $0''{\cdot}3$ in north polar distance; he then says:—

"I have added to this table the factors of solar motion, computed on the supposition that the sun is moving towards a point in the heavens defined by the Æ 260°, and north polar distance 55° 37′. The fact of the existence of this motion is now so fully established by the separate investigations of Messrs. Argelander, O. Struve, and Galloway, and the approximate value of the amount of the solar motion deduced by M. William Struve is deserving of so much attention, that I have thought it worth while to compute these factors, and to place them side by side with the observed proper motions of my third table; and a bare inspection of the related columns will suffice to shew us that the apex of solar motion is at least pretty accurately determined, while the factors themselves will be useful in a verification of its position." (Memoirs of the Royal Astr. Soc. vol. xix. page 133.)

In the Cycle (vol. II. page 391), I mentioned moreover that our system was travelling *slowly;* that expression, however, must only be taken comparatively, because it may require thousands of centuries before a single revolution of the solar orbit is completed. By recent results it is computed that we are moving on at the rate of 154 millions of miles annually, or upwards of 17,000 miles in every hour of time! The argument is thus summed up by the elder Struve in his Etudes d'Astronomie Stellaire, page 108, published in 1847:—

"The motion of the solar system in space is directed towards a point in the celestial vault situated on the right line that unites the two stars of the 3rd magnitude π and μ Herculis, at a fourth of the apparent distance between them starting from π. The velocity of this motion is such, that the sun, with all the bodies depending on him, advances annually, in the direction above stated, 1·623 times the radius of the terrestrial orbit, or 33,550,000 geographical miles.* The probable error in the numbers here given may amount to 4,733,000 geographical miles, or to one-seventh of the whole. We might therefore wager 400,000 to 1 in favour of the reality of a progressive proper motion of the sun, and 1 against 1 that it is comprised between the limits of 38 and 29 millions of miles."

But there is no immediate cause for dread or alarm: since, even under the largest unit, the solar system cannot pass the enormous interval in less than a couple of millions of years!

* These figures are in German miles, which by Professor Littrow's interpretation are each equal to 4·6 of our truly-ascertained miles. The reduction of Struve's numbers therefore yields the following amount, roundly in English geographical miles—

Solar progression	154,330,000
Probable error	22,000,000
Limits of solar velocity . . .	176,000,000
	132,000,000

I have stated in the Cycle (I. page 282), that the exact direction and nature of the mysterious path we are pursuing must necessarily be left for men of after-times to determine; since so vast is the orbit that all the stellar displacements hitherto detected only represent it as a right line, the which is utterly incompatible with the principles of motion and gravitation. It must be curvilinear, and M. Mädler has made this hypothesis the basis of some very ingenious speculations, in which he assumes that Alcyone, the lucida of the Pleiades, is actually the central body which controls us, and around which we—sun and attendants—are revolving. The idea is magnificent and warmly treated; but from the want of a firmer foundation, and other obvious causes, it is equally unsatisfactory and inconclusive. These matters will be much better understood and known a handful of ages hence.

Mercury (Cycle, I. pages 95—103). As there was full reason to predict in the first volume of the Cycle (page 252), so the mass of this planet has been more accurately approached by the perturbations endured by Encke's comet, than it was when I wrote: but although I considered, and still consider, the evidence to be rather shaky, I ought to have mentioned Schrœter's conclusions as to the heights of some Mercurian mountains, both in regard to the assertion, and in full recognition of his indefatigable perseverance on a body, the constant proximity of which to the solar blaze so greatly interferes with stringent scrutiny. The astronomer of Lilienthal conceived that he had a clear proof of the existence of mighty asperities on the surface of Mercury: but surely his inferences require confirmation before they can be assumed as facts, or recorded among astronomical truths. The point in question stands thus:—a certain blunting of the southern horn of its crescent—seen and observed with infinite attention—was imputed to a most obvious cause, namely, to the light of the sun being intercepted by a high mountain. This is a fair preliminary condition; but as the height calculated by the extent of the truncature is upwards of twelve miles, or nearly the 125th part of the planet's radius, the result appears to require modification. Aware of the responsibility incurred, he estimated for comparison the height of Chimborazo, which when

I was in the Pacific (1807) was esteemed the loftiest mountain of our globe, at $\frac{1}{1017}$ of the earth's radius; which is a vast disproportion when the relative magnitudes of these two bodies are taken into the argument.

Notwithstanding that Schrœter assigned a period of rotation for Mercury, and might thereupon have assumed that its figure was not truly spherical, yet he confesses that he could not detect the slightest trace of compression or ellipticity in the form, nor indeed could Sir W. Herschel, though armed with superior means; but, on the occasion of the transit of Mercury over the sun's disc in November 1848, the Rev. W. R. Dawes, by his very delicate micrometric measurements established a difference of $\frac{1}{29}$th between its polar and equatoreal diameters. I happened to be in the chair of the Royal Astronomical Society when this interesting announcement was made (8th December, 1848,) and I recommend those who have not already read his paper to consult the Monthly Notices of that Society, vol. ix. pp. 21 and 22, where they will find his means and method for arriving at the result.

Venus (Cycle, I. pp. 103 to 111.) A few details must be added to my account of this beautiful planet, as the transits alluded to constitute a memorable epoch in the astronomy of the eighteenth century, and, relative to that phenomenon in 1761, some more official papers in aid of its history have turned up. At page 110 of the above cited volume, I mentioned the transit of Venus which occurred in 1639, and the next following as in 1761. But I was startled, on receiving a catalogue from Mr. Sotheby, the well-known auctioneer, at there being among the historical papers of Captain Warrington, R.N., to be sold on Monday the 2nd of August, 1858, the following announcement:—

No. 205. George II. The King's warrant signed to pay Peter Duvall, Vice-President of the Royal Society, eight hundred pounds to enable them to send proper persons to various parts of the globe to observe the transit of Venus over the sun, 1727. Countersigned by *Holles Newcastle*, *F. B. Legge*, *James Oswald*, with the print of the transit, &c.

Here I was taken flat aback; still, from its being under such official authority, it became a duty to look to it, and the only conclusive method of knowing what it all meant was to purchase the paper. Not a moment was lost in procuring it, and running an eager eye over its contents; but, though docketed 1727,

that was only in allusion to the office-number of the warrant on which they acted for payment, but the phenomenon really treated of was not expected till 1761. Yet this document is valuable as showing the desire of attending to what had "only been seen once before since the world began;" and it proves that, though George III. has had the whole credit of fitting out the Transit-of-Venus expeditions, a portion of his claim in that respect must be restored to his royal grandfather. It is indeed no great matter, since the Royal Society, aided by outward pressure, pulled the strings; but, to set the affair in the clearest light, as all topics worthy of record ought to be, I will here lodge a copy of the King's warrant:—

George R.

Whereas it hath been humbly represented to us by the President, Council, and Fellows of our Royal Society of London that the reputation and honour of the British Nation require some diligent, faithfull, and skilfull Persons to be sent from this our Kingdom to proper parts of the Globe in order to make exact observations of the transit of the Planet Venus over the Sun, which rare Phœnomenon will happen on the 6th of June in the year 1761: And Whereas We have resolved to give the sum of Eight hundred pounds to be applied by Our said Royal Society for the purpose aforesaid: Our Will and Pleasure therefore is that, by Virtue of Our Generall Letters of Privy Seal, bearing date the 26th day of June, 1727, You issue and pay, or cause to be issued and paid, out of any our Treasure or Revenue in the receipt of Our Exchequer applicable to the uses of Our Civil Government, unto Peter Duvall, Esqr., Vice President of the said Society, or his Assigns, the sum of Eight hundred pounds without account, to be by him or them paid over and applied for the purpose aforesaid under the direction of the said President, Council, and Fellows. And for so doing this shall be your Warrant. Given at Our Court at Kensington this 21st day of July, 1760, in the 34th year of Our Reign.

Holles Newcastle

H. B. Legge

To the Commissioners of our Treasury.

James Oswald

This sum, reduced by hungry red-tapeism to 759*l.* 6*s.*, was received from the Exchequer by Mr. Peter Duvall on the 5th of August, to enable the Society to send the Rev. Nevil Maskelyne, afterwards Astronomer Royal, to the island of St. Helena; and a similar sum of 800*l.* (758*l.* 16*s.* received) was granted to the same body that they might despatch Messrs. Mason and Dixon to Bencoolen; and Dr. Birch reported, on the same 5th of August, that Mr. Cleveland, the Secretary of the Admiralty, had informed him that My Lords had ordered a ship (the Sea-horse frigate) to be prepared for conveying the observers to their respective destinations. Owing to certain obstructive dock-yard delays Maskelyne would not wait for the frigate, but, embarking himself with his instruments and comforts* in another vessel, he arrived at St. Helena within the appointed time. There he made every preparation, though on account of cloudy weather he reaped little that was satisfactory of the remarkable transit; his voyage, however, proved eminently useful to navigation, especially as relates to what are now so well known as lunar observations.

The Bencoolen expedition was still more unlucky, for it did not quit the shores of England till January, 1761; and, when at last the Sea-horse left Spithead, she had hardly cleared the Start Point before she fell in with Le Grand, a French frigate of superior force. An action of course ensued, and was continued with great spirit on both sides, till, another of our ships heaving in sight, Le Grand made all sail and escaped, after having killed 11 of the

* These comforts may be understood by a glance at the estimate which Maskelyne drew up, and which was approved by the Council. Besides the *payement honoraire* of 150*l.*, the following was granted for his necessary expenses, in which the sums for drink prove that he was not quite what is now yclept a Tee-totaller :—

	£	s.	d.
Boarding at St. Helena at six shillings per day for one year	109	10	0
Liquors at five shillings per day for same time	91	5	0
Washing at ninepence per day	13	13	9
Other expenses and incidental charges at one shilling and sixpence per day	27	7	6
	241	16	3
Liquors on board of ship for three months going and three months coming back	50	0	0
Total expenses for one year	£291	16	3

Sea-horse's crew, and wounded 38. This *contretems* compelled Captain Smith to put back for repairs into Plymouth, from whence the Royal Society received a dismal Jeremiad from the disconcerted *savans,* in which they made a stern-board and declined their mission; but the following unmistakeable remonstrance brought them to their senses:—

The Council are extremely surprised at your declining to pursue your voyage to Bencoolen, which you have so solemnly undertaken, and have actually received several sums of money upon account of your expenses and in earnest of performing your contract. That your refusal to proceed upon this voyage, after having so publickly and notoriously engaged in it, will be a reproach to the nation in general, and to the Royal Society in particular; and, more especially and fatally, to yourselves: and that, after the Crown has been graciously and generously pleased to encourage this undertaking by a grant of money towards carrying it on, and the Lords of the Admiralty to fit out a ship of war, on purpose to carry you to Bencoolen, and after the expectation of this and various other nations has been raised to attend the event of your voyage; your declining it at this critical juncture, when it is too late to supply your places, cannot fail to bring an indelible scandal upon your character, and probably end in your utter ruin.

In case you shall persist in your refusal, or voluntarily frustrate the end and disappoint the intention of your voyage, or take any steps to thwart it, you may assure yourselves of being treated by the Council with the most inflexible resentment, and prosecuted with the utmost severity of the law. To prevent all possibility of doubt concerning your undertaking the voyage, or omitting to go, the Council do absolutely and expressly direct and require you to go on board the Sea-horse, and enter upon the voyage, be the event as it may fall out.

This heavy shot between wind and water, told well; but enforced service is seldom productive of goodly fruit. The transit of Venus gained little by the return of the malcontents to obedience, for, so much time had been expended in refitting the frigate, that, apprehensive of not reaching Bencoolen in sufficient time, they brought-to at the Cape of Good Hope, and made their observations there. The results are briefly alluded to in the Cycle; but for an able dis cussion of the whole, see Encke's treatise on the subject.

Though the conclusions were not so definite as science required, the advent was a capital preparative for 1769; and, moreover, the recurrence of a phenomenon which had only been once seen before "since the world began," aroused general attention; and various eye-witnesses are on record, to whom I will here add another. Being on a visit to Tarn Bank in October 1858, the hospitable owner showed me a long series of volumes of yearly memoranda kept by his great-grandfather Isaac Fletcher, of Underwood near Cockermouth.

He was a Cumberland yeoman of moderate education, but a man of observation and extensive reading. Under date of June 6th, 1761, we found this entry:—

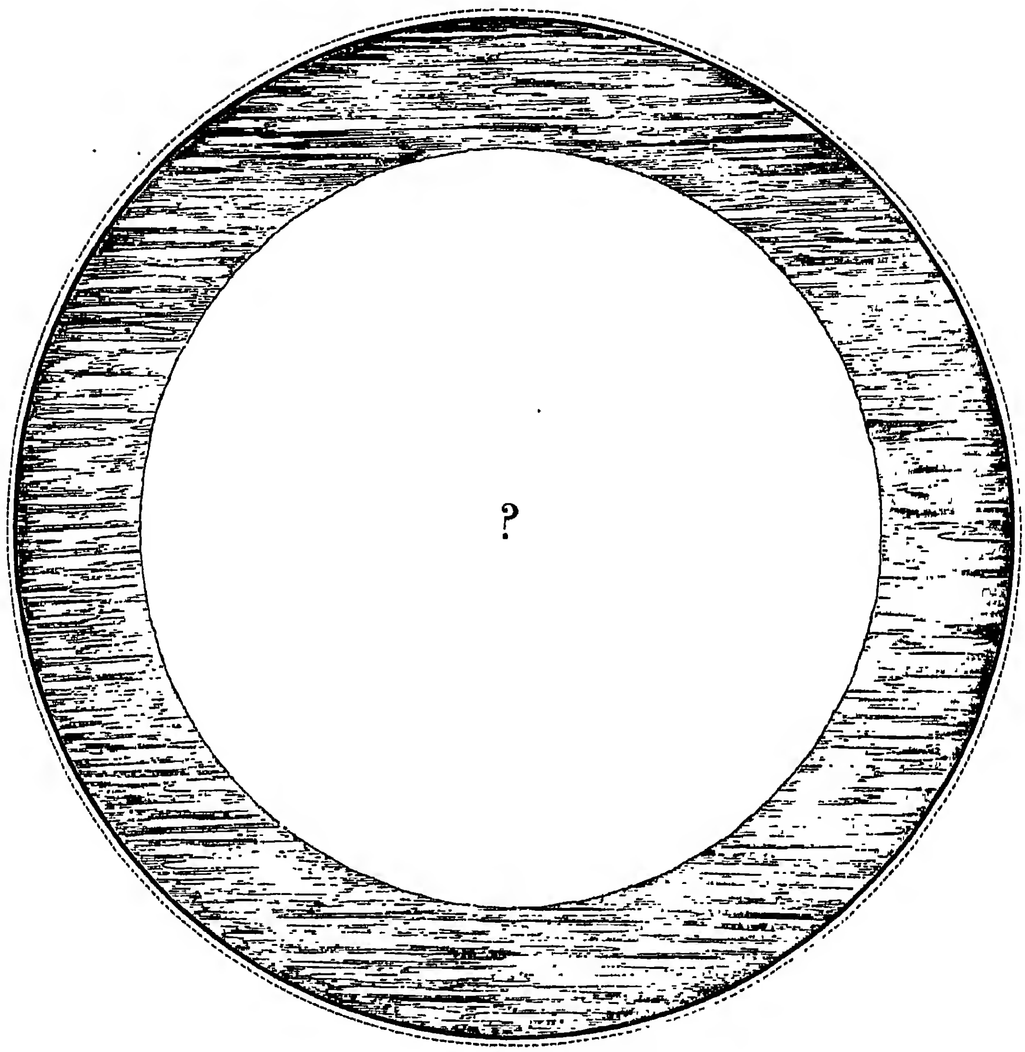
This morning, about four, observed the transit of the planet Venus over the sun's disc with a solar telescope. The sun then appeared large, with a thin black cloud over it, which soon went off as the sun rose higher in the horizon. The image of the sun and the planet Venus as a black spot appeared very plain upon a white cloth put up for that purpose. The path of the planet over the sun's disc was easy to delineate, and very curious to behold, though clouds interfered. It went off the sun about 35 minutes past eight in the morning, soon after which thick clouds covered the whole hemisphere most of the day, with some showers of rain in the afternoon.

I have not been fortunate with Venus myself; nor after repeated gazings have I been able to add more about her telescopic aspect than what is already stated (Cycle, I. page 108). To be sure, in consequence of the notice given in the Royal Astronomical Society's Monthly Notices, (vol. xiv. page 224), I watched her inferior conjunction in 1855, but only perceived a fine silvery crescent without any indication of an annular fringe of light surrounding the dark part of the disc. Nor have I at any time been able to make out the spots, which my correspondent De Vico saw so frequently at Rome. Here some one might say that the difference was owing to the admirable transparency of the exquisite Italian skies; but such assertions are better suited to the novelist than to the astronomer. I have frequently been annoyed in hot countries with the unsteadiness of the stars during observation; and in my Cycle (Vol. II. page 128) I published the remark made to me on this subject by the thoroughly-practised Piazzi. Now, this very remark is quoted by Padre Angelo Secchi, the successor of De Vico, in his recent Descrizione del nuovo Osservatorio del Collegio Romano; wherein he arraigns the weather again and again.*

* His exact words (p. 31) are—"Ho accennato sopra alla difficoltà che s'incontra nel prender misure esatte delle stelle doppie, dal continuo moto dell' atmosfera. Di questa si lagnano tutti gli osservatori, ma parecchie nelli paesi più Settentrionali sia tale agitazione dell' aria meno sensibile. L'ammiraglio Smith nella sua bell' opera *Celestial Cicle*, vol. ii. p. 128, ricorda a questo proposito la noia che ne riceveva il famoso Piazzi, e come esso per ciò preferiva assai Greenwich alla sua Palermo."

He then produces Biot as complaining of the irregular refractions in the Italian climate, and, reasoning upon this, pronounces that the greatest astronomical achievements have been performed in England—"Quindi si comprende come in certi paesi creduti poco proprii alle osservazioni astronomiche, pure siansi compiti i più grandi lavori della scienza, quale è per esempio l' Inghilterra."

The Earth's Crust (Cycle, I. page 117.) Since my allusions there to Geology were printed, there have been numerous valuable discussions, as well as some dreamy reveries, on the Crust of the Earth; which, from a mathematical examination of the velocity of our rotation, is concluded to be subject to local variations in all likelihood correspondingly unequal on its inner surface. By mining and other artificial means of penetrating this shell, a gradual augmentation of heat is encountered, increasing proportionally to the depth: in other words, without looking to great numerical precision, if the temperature continued to rise at the mean rate of 1° of Fahrenheit in every 80 feet of depth, which is a fair average from the experiments hitherto made, it would become probable that the earth is a spherical bomb, having its interior still in a state of fusion by heat. At a depth of, say, 50 miles, the temperature would be above twice that which would suffice to melt iron, and still more to destroy cohesion in any other matter composing the said crust. The average density of the earth, as deduced from several independent authorities, amounts to five and a half times that of pure water, or double that of the average afforded by the earths and rocks on the terrestrial surface, whence it is evident that much of the interior must be metallic, or immensely condensed. Since Perkins gives the density of water as doubled by the pressure of 200 atmospheres, it must either be compressed into a solid at the earth's centre, or enabled to retain its fluidity by an exceedingly high temperature. To these data we may add that the densest metals increase in density by hammering, and that fluids are more compressible than solids. Astronomy also supplies arguments in favour of the density increasing as we approach the centre, if we may judge by the evidence yielded by the moon; and, returning to our own globe for practical examples, we may adduce the influence of Mount Schehallien, in Scotland, on the plumb-line, amounting to 11·7″ in observing zenith stars, though the actual distance through the base of the mountain was under 5000 feet. This perplexing yet truly important problem at present may be viewed thus:—



Supposing this diagram to represent a sphere about 8,000 miles in diameter, the occult or dotted outer circle would indicate the height of the atmosphere, dilated by rarefaction up to vacuum; the mere thickness of the black circle would cover, or equal, the utmost heights and depths of the land and sea; while a similar line inside, would suffice for the whole extent that we can accurately refer to. Here the shaded portion would represent the utmost depth which human inference has yet assigned for the terrestial crust; but what do we know of the rest? By the surprizing revealments of geology an advance is

made, yet only to a comparatively small vertical extent. In granite and other igneous rocks, which have been forced up from the unknown regions below, no living organism is indicated to have existed—they are destitute of any relics of animated nature. These formations may exceed 20 miles in thickness;—but must we assume that all below this estimated depth is molten matter? The strata in which fossils are found imbedded, do not altogether perhaps reach down more than 15 miles. Can we then even venture to guess whether the central vastness be a solid body of matter, infinitely condensed by enormous pressure like Dr. Young's steel, or even like Newton's atoms,—or whether, with probability on its side, the cavity be filled with fluids, so full as not to allow of any waves?—hence the safety of a barometer tube when the cistern is screwed up tight.

While drawing the foregoing figure, and assuming the shadowed portion to be 800 or 1,000 miles thick, according to the deductions of my friend Mr. Hopkins, as gathered from his communications to the Royal Society (1839—1842), an omniological straw-splitter at my elbow asseverated that the Germans had found the earth's crust to be exactly 26½ miles in thickness. Now, though this question might apparently be very well ranged among the *arcana naturæ* utterly impervious to our present perceptions, the deduction was affirmed so positively, that I referred it to Mr. Hopkins, as the cautious, comprehensive, and scientific Præses of this department. His letter from Cambridge, dated 21st March, 1858, ran thus:—

"Don't believe your omnium-gatherum friend. Neither the Germans nor any other people under the sun *can* have found the earth's crust only 26 miles thick. As you suggest, they (*i.e.* whoever makes such a statement) ought to give it in *feet* and *inches*. The thing would then be complete of its kind.

"The lowest that can be made of the thickness in question is founded on the hypothesis that the heat of the crust is due entirely to central heat, and moreover that the conductive power of the rocks below the sedimentary rocks is no greater than that of these latter rocks. I am now engaged in experiments which show this to be wrong. If both the aforesaid hypotheses were true, the crust might possibly be estimated at some 50 miles. I have now a memoir passing through the press containing a great number of experiments bearing on the subject, and shall have two others before long, when I have completed my experiments. I shall then resume the subject. In the mean time I have seen nothing to shake my conviction in the truth of my former conclusion.

"I have said, that, according to my solution of the problem of the Precession of the Equinoxes, the minimum thickness must be about a fourth or fifth of the earth's radius. I don't mean to say that it might not possibly be thinner than that, as 4 or 500 miles for instance; but I am thoroughly convinced that it cannot be anything like so thin as 100 miles, for example. When my Memoirs come forth I will send them to you. My investigations impose no limit to the maximum thickness.

"The most important point, geologically, is whether the crust is thin enough to allow the *direct* influence of a fluid incandescent nucleus on the phenomena of volcanoes, elevations, &c. I am thoroughly convinced IT IS NOT."

Under this comfortable assurance, a study of the relations of the foregoing diagram opens a marvellous field for speculation. As the earth's crust consists mostly of oxidized bodies, may there not have been a great absorption of atmosphere,—itself a powerful absorbent of light and heat—in the process: and may not the said crust, acted upon by a high internal temperature, possess a degree of beneficial elasticity? Are not light, heat, magnetism, whether in the state of galvanism or electricity, striking modifications of a mighty and all-pervading principle as yet comparatively unknown? Another Newton may one day arrive at conclusions which will then be rendered obvious enough. Even now, amidst the struggles of intellectual fancy, the facts cannot be questioned, though some of the deductions may be at sea. Thus, we are told that, were the particles of light the size only of the 12,000th part of a grain of sand, they would batter and demolish our world; yet if light be a material agent, liable to attraction from ponderable matter, a question would ensue as to what becomes of the infinite quantity thrown upon the earth's surface. However, to prove that speculation is still adrift relative to our main argument, let us now listen to Arago (Popular Astronomy, Book xx. Ch. xviii.)—

"Admitting the constancy of the progressive increase of temperature in proportion as we descend into the interior of the earth, we should find that, at the depth of eight or nine leagues below the surface of the earth, that is to say, at the depth of only four or five times more than the elevation of our highest mountains, those substances that we know to be most capable of resisting the action of heat would be in a state of fusion. In fact, in a letter from M. Mitscherlich to my friend Alexander Humboldt, I read: 'The temperatures requisite for fusing metals have been very much exaggerated. The flame of hydrogen burning in air is only 1560° on the centigrade scale (2808° Fahrenheit). In this flame platinum fuses. Granite fuses at a temperature below that of soft iron; it fuses at about 1300°, and silver at 1023°, (2340° and 1841° Fahrenheit.) By supposing

an increase of 0·033° centigrade in every meter (*nearly* 0·02° *Fahrenheit for every foot of depth*) we should find at a depth of 40,000 meters (131,236 *feet*) (24·8 *miles*) a temperature, in round numbers, of 1320° (2376° *Fahrenheit*). Then granite becomes fluid there.'

"Thus 40,000 metres (25 *miles*) is the approximate measure of the thickness of the earth's crust. This is a deduction from observations which, unfortunately, have hitherto been practicable only to the slight depth of 650 metres (2,133 *feet*), yet it suffices to explain the reaction exerted against the weak parts of the solid envelope of our planet by the internal fluid matter. The existence of volcanoes is thus easily explained."

Plausible as these data may appear, before drawing conclusions we should consider the probable obstructions to fluidity under enormous pressure: even on the small scale that man can command, water is prevented from boiling. Metals soon begin to expand with heat, and thereby undergo increased compression if inclosed in an unyielding case, such as the earth's crust may prove to be in regard to its nucleus.

On that same page of the Cycle (117) when I alluded to the Tropical year, I might have said that its length in sidereal days is 365·2396, or $365^d\ 5^h\ 45^m\ 1{\cdot}44^s$; and the several determinations of its duration may be exhibited thus in mean solar time:—

	d.	h.	m.	s.
Albategnius, about A.D. 1000	365	5	46	24
Copernicus, in 1543	365	5	49	06
Tycho Brahe, in 1602	365	5	48	45
Kepler (*Tabulæ Rudolphinæ*)	365	5	48	57·6
Flamsteed (*passim*)	365	5	48	57·5
Halley (*Astronomical Tables*)	365	5	48	54·8
Bessel (*for the year* 1800)	365	5	48	47·8
Airy (*communicated* 28*th January*, 1859)	365	5	48	46·05*

Relative to the succeeding page (118) it may be well to add here, that I might have led off, in the list of ascertained obliquities of the Ecliptic, with

* The Astronomer-Royal's lively letter runs thus:—"On examining the fundamental numbers of Le Verrier's Tables (the latest authority), I have computed, from his elements of different kinds, the length of the Tropical year, and find it as follows:—

Mean Solar Time, $365^d\ 5^h\ 48^m\ 46{\cdot}045440^s$.

I am sorry to be unable to give you a greater number of decimals. But, perhaps, these will suffice for a Gardener's Calendar for a year or two."

that of Tcheou-Kong, determined about B.C. 1100, to be 23° 54′ 02″; especially as the Chinese observations were taken with regular instruments—gnomons or armillæ.

THE MOON (Cycle I. pages 95 to 103). Since 1844, our satellite has been as fully gazed at by me, under several phases, as an object-glass of $5\frac{9}{10}$ inches would admit of, besides having had occasional snatches through powerful reflectors; and it is assuredly a body on which the mind and vision never tire. Still, after all, the recent practical attempts have not been very productive of valuable results, nor have they greatly enlarged our knowledge respecting its surface; but that is not the case with the purely analytical branch of this noble science, for my learned friend, Professor Hansen, of Gotha, has lately been so fortunate as to detect two sensible inequalities in the lunar motions, one of 239 years' duration, and the other of 273, which have, humanly speaking, perfected the Tables of the Moon. Lord Rosse's giant instrument—which may be said to bear efficiently a power magnifying upwards of 5000 diameters, to the wonderful amplification of surface—makes our satellite resemble a globe of molten silver; but, in detail, only shews an increase of the extinct volcanoes, and frightful ruggedness, which I have described as one vast ruin of nature (Cycle, I. pages 130 to 132). These features engaged much of my attention, yet the only conclusion to be arrived at is a notion that they are owing to volcanic action and moon-quakes. Many of the ring-formed ridges are like bursten bubbles of boiling pitch, or aeriform discharges blown off from fused metals; while in others, the annular character observable in their plan loses much of its analogy to like formations on the earth's surface. Thus the gigantic ring-mountain, Tycho, is elevated above the surrounding region about 12,000 feet, but its interior area is an inclosed plain of nearly fifty miles in diameter, with a depth of 16,000 feet.

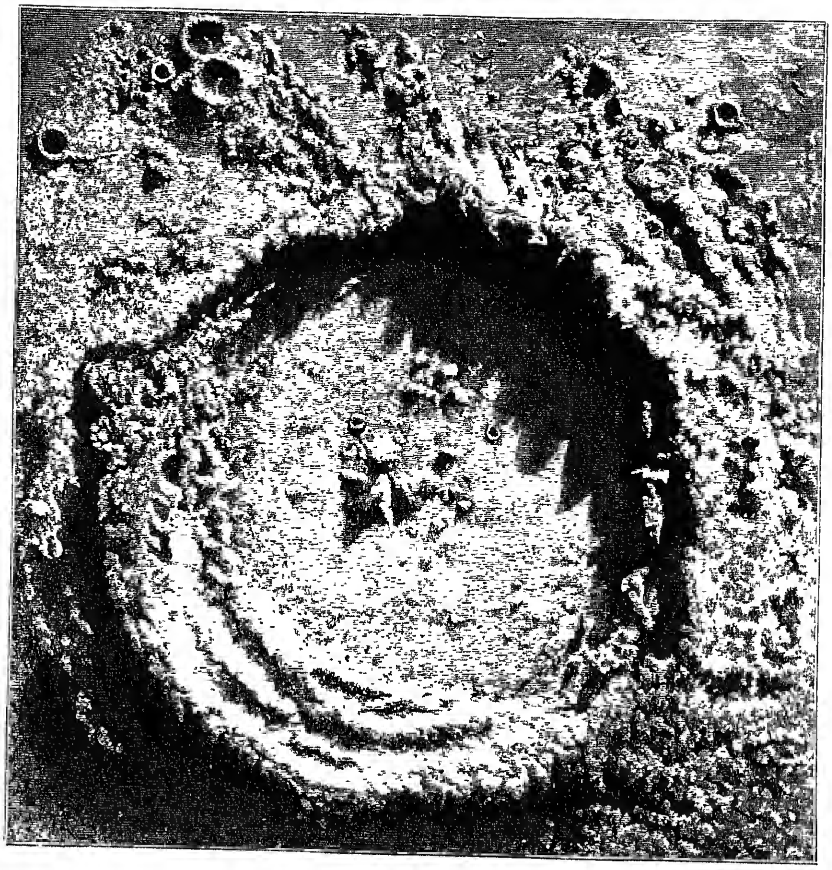
The radiating streaks (Cycle I. page 128), so conspicuously brilliant at the time of full moon, appear to be cracks or gullies, that occurred when the cavity of Tycho first convulsed the lunar crust, and may since have been

filled up with some highly reflective lustrous substance.* My zealous friend, Mr. James Nasmyth, has devoted intense attention to the lunar ring-mountains, and on the whole concludes that the circular walls are produced by eruptions having broken forth at one point, and scattered the ejected matter round the central orifice with equal force in all directions. From the small amount of gravity in the moon, the scoriæ, and even lava, could easily be thrown to greater distances than in larger globes; while the central peak, so observable in most of the annular mountains, he thinks due to the expiring energies of volcanic action: and he considers that the brilliant streaks issuing from Tycho, Aristarchus, and other mountains, are owing to immense fissures, through which the bright molten lava has oozed, and is thinly spread over their banks.

Here the mention of certain grooves resembling scratches (and therefore called *rainures* by the French) must not be omitted. They sometimes exceed a hundred miles in length, though only about three quarters of a mile in breadth, and are alternately dark or bright according to the angle at which the sun's rays fall upon them. The fertile imagination of Schrœter, who only discerned two, suggested them to be proofs of intelligent inhabitants; but subsequent observers have traced seventy, and their course, sometimes ending at a crater, and others continuing their way across the cavity, is perplexing.

That superb specimen of its order, Copernicus, is more than fifty miles in diameter, with cliffs of 11,000 feet above the interior surface; and the ridges, rills, gulfs, valleys, pearly-peaks, mountain-terraces, and irregular but radiant streaks, are unmistakeably marked even in the glittering nimbus around. Struck with the importance of so noble a lunar feature—one still demanding the especial attention of the wielders of powerful telescopes—I applied to Mr. Nasmyth for a drawing of the crater, and being thereupon favoured with a faithful monograph representation, I procured the annexed reduction of it from the accurate burin of Mr. Thomas Cobb:—

* Baron Waltershausen, who spent some years on Etna, and there proved several of my points, showed me his drawings of some very remarkable trap-dykes, radiating from the great crater of the Sicilian volcano.



I ought perhaps to have premised that Nasmyth—like his own powerful steam-hammer, which can either smash an anchor or only crack a filbert—compassed a siege of the moon with gigantic reflectors, his own handiwork, as well as a reduction of her rugged aspect to delicate delineation. Thus animated, he constructed two Newtonian telescopes, one of 8¾ inches aperture, and 9 feet focus—the other of 12 inches aperture with 13 feet focus; and these were sedulously employed for several years, principally under powers magnifying from 240 to 360 times,—with what good effect may be seen in his numerous diagrams, and in his essay on the telescopic appearance of the Moon (Royal Astronomical Society's Memoirs, vol. XV. pages 147 to 156). In reasoning on the nature and structure of lunar craters, he submits an engraving of Gassendus, by no means one of the largest; and as—from its aspect varying

greatly under the different phases—I am interested in the same spot, he kindly lent me the block from which the accompanying impression is taken :—

Gassendus he describes as "measuring about 60 miles in diameter, near to which may be seen a minute crater marked *o*, and another marked *x*; *o* being about the size of our Vesuvius, while *x* is fully larger than Etna! A comparison between those and the large one will convey perhaps a clearer idea of their relative magnitudes than any attempt at description." On other counts also this is a truly wonderful object; the interior being rough and bright, with its parasites C and D, and its several ridges and depressions; it resembles a circular wall on the whole, the western side of which rises to the height of 12,000 feet above the plain of the area, with enormous steepness, since the interior remains free from shadow only five or six days during full moon. There is a small passage at the southern part, leading into the Mare Humorum, of which Mädler and Beer describe Gassendus as the northern key-stone, and another strait communicating with the large crater at the north, while a crowd of peaks rise in the centre, and bright mountain-masses close the east.

There are various indications of a relationship between the planets, and there is an inferential evidence in their fluid origin, and subsequent solidifi-

cation: and among other instances their apparent connection in astronomical and geological phenomena ought not to be overlooked. It is not, however, that so interminable a point need be discussed here, further than to submit, after the above lunar formations, the following terrestrial specimen. It faithfully represents the great crater of Teneriffe—eight miles in diameter, with its parasitic cones and peak, the latter 12,198 feet high—on a scale $\frac{1}{849000}$ of nature (see the Admiralty Teneriffe Report of 1856, page 574):—

The Mare Imbrium is one of the smaller lunar seas, yet is 260 miles from north to south, by about 290 from east to west. It is surrounded by steep elevations; and there are numerous craters, with long marked veins, visible on its surface, which is of a decided green tint, and upon it the white efflorescences formed by the *blow-holes* are well seen. The irregularities on this, and other lunar oceans, have made some observers leap to the conclusion that they are large tracts of fertile land; but Arago thinks they may still be slightly covered with water, the light reflected from the beds of such lagoons being more intense than that proceeding from the surface of deeper water. Besides the foregoing representation, Mr. Nasmyth gratified me with a beautiful drawing of Gassendus, on a larger scale, accompanying his esteemed present with a real

good-will offer—in case the Cycle was coming to another edition—to act as my "lunar limner," and delineate any part or parts of "ye Moone her surfis;" and another zealous selenographer, hight Warren de la Rue, made me a tender of the use of his as yet unexcelled planetary plates. The productions of these gentlemen, so faithful in the leading physical and mechanical features, would indeed have been a treat to Galileo, albeit he used his little "optic tube" to good purpose in making out moon-mountains at all.

Gassendus is situated at the pitch of the Apennine range, the most remarkable of the lunar mountain-chains, with cliffs upwards of 13,000 feet high, and its culminating peak Huyghens is 18,000 feet above the Mare Imbrium. It is a wonderful field, for here telescopic scrutiny is rewarded with a striking series of rocky ridges and gulleys, elevated plateaux and vast cavities, bright dykes and dark plots, pyramidal peaks and chaotic masses, forming grand landscapes, possessing apparently much character in common with our Alps, Andes, and Himalayas. The nearest summit of this group, towards the well-formed annular and frosted argentine walls of Eratosthenes, is called Wolf by Beer and Mädler, by whose micrometric measures it is 11,034 feet in height. And, not to be misunderstood, I should here state that, in the expression (Cycle, I. p. 131) of the lunar mountains, with certain exceptions, being of moderate elevation, it was rather relying on the deductions of ŋ than those of others; but Beer and Mädler (*Der Monde*) restore in some degree the altitudes formerly assigned by Schrœter. Thus, though it was then said that the Apennines formed the highest range (18,000 feet), I ought to have mentioned there being several peaks of still greater height, as the ring mass named Casatus, 22,800 feet high, and Newton, which is said to reach a thousand feet higher still: this is probably the most elevated on the face of the moon, for a shade of doubt has been thrown over Doerfel's boasted 24,944 feet, on account of its lying at the margin of the limb. These heights, unlike the proportions of our mundane protuberances, are so great for their location, that the summits can never lose sight of the sun, except during eclipses; and, compared with the size of the spheres on which they stand—the diameter of the moon being

little more than one fourth that of our globe—the lunar mountains, even under possible exaggeration, are on a grander scale than those of the Earth.

Among other contributions to lunar physics, I am happy to cite the three phases of the Mare Crisium and its shores by Professor Piazzi Smyth, which exhibit that region under three several directions of incident light—new moon, full moon, and old moon,—reduced to a mean state of libration. These views were taken by my son, at the request of the British Association for the Advancement of Science, in 1854; and it is to be hoped that, ere the present century shall pass away, the whole visible surface of our satellite will have been thus delineated (see Edinburgh Observations, vol. xi.)*

These marked ranges of elevated districts, and the smooth regions between them assumed to be seas, occasion the well-known aspect which has been regarded—in most ages and countries—as a resemblance of the human face. In classic times those shades were deemed to represent the effigies of a handsome female, as Artemis, Selene, or Sybilla.† The features are thus described by Agesianax, a Greek poet, of whom nothing has been preserved but the three didactic lines cited by Plutarch—*De facie in orbe Lunæ,* which may be thus rendered from Xylander's Latinized version:—

Over the orb shines a resplendent light,
In midst of which a damsel's face is seen;
Whose cheeks, suffused, display her blushes bright,
Her eye cerulean, or a pale sea-green.

The which is certainly a more pleasing notion than that which crept into mediæval parlance; namely, that the face pertained to a hoary sinner, who was banished thither for picking up sticks on the sabbath. While some held

* I ought not to omit mentioning a beautiful model of the moon on the small scale of 12 inches, but wonderfully exact, which was entrusted to my charge by Sir John Herschel in 1846. It was constructed by Madame de Witte, a Hanoverian lady, who made a general outline from Beer and Mädler's great engraving, and then filled in the details from her own telescopic observations. During the time that it was in my care, I consulted some electrotypists as to the feasibility of multiplying copies of this elaborate gem, but they all shrunk from the task.

† These early allusions may have influenced the fancy of the Dutchman, who published a book in 1704, intituled Ὅμηρος Ἑβραῖος, in order to prove that the subject of Homer's two poems is taken from the Scriptures,—the Iliad treating of the taking of Jericho, and the Odyssey the adventures of the Israelites till the death of Moses. Surely Gerard Croes and Père Hardouin had kindred souls!

this to refer to him who was stoned to death (Numbers, xv. 32—36), others, among whom was Dante, firmly insist upon the likeness being that of the wicked Cain—*Caino e le spine*. The popular superstition of the old man and the faggot is alluded to by Chaucer, and by Shakespeare; but Milton, too sublime for "pycchynd stak," merely touches the physics:—

> and as lowest first the Moon;
> Whence in her visage those round spots, unpurg'd
> Vapours not yet into her substance turn'd.

Although in the Cycle I dwelt not upon lunar influences—which, as Johnson observes, have a great power in vulgar philosophy—I have sojourned too long in intertropical regions to assert myself to be entirely sceptical on the point, especially as the power of lunar light in intenerating flesh meat, and affecting sleepers under exposition to its rays, is sufficiently patent; although jumpers at conclusions declare that the one is caused by a deposition of dew, and the other is a result of the temperature of the skin being lowered by radiation whilst in an unresisting state. But, on the contrary, I place no reliance on the assertion that a red moon, however it may prognosticate wind, exerts, or can exert, any injurious influence upon the phenomena of vegetation (Cycle, I. page 124); or that its phases should regulate the times for felling timber. The occasional finely-tinted disc of our satellite which gave rise to the proverb for rare events—'once in a blue moon'—a phenomenon I have more than once noted, may be complemental of the former colour, though it seems that, if a very vivid red is placed by the side of white, it is the latter which assumes a bluish hue. Such phenomena led to the question as to whether the moon absorbed all the solar heat radiated to her, or whether, with the light which she transmits to us, there is not an almost infinitesimal quotum of heat as well. Until lately, no satisfactory answer had been given: indeed it was for ages thought that the lunar rays were frigorific, at least it was held to be proved that no heat existed in them (Cycle, I. pages 122 and 123). It was a mistake.

Many years have passed since Sir John Herschel communicated to me his belief that full moons are generally accompanied by clear skies; this may be

owing perhaps to the circumstance now revealed, namely, that at those times the clouds may be dissolved in the higher regions of the atmosphere by the influence of this warmth of the lunar rays. Sir John's later expression (Outlines of Astronomy, 1849, page 261) runs thus:—

"Though the surface of the full moon exposed to us must necessarily be very much heated,—*possibly* to a degree much exceeding that of boiling water, yet we *feel* no heat from it, and even in the focus of large reflectors it fails to affect thermometers. No doubt, therefore, its heat (conformably to what is observed of that of bodies heated below the point of luminosity) is much more readily absorbed in traversing transparent media than direct solar heat, and is extinguished in the upper regions of our atmosphere, never reaching the surface of the earth at all. Some probability is given to this by the *tendency to disappearance of clouds under the full moon*, a meteorological *fact* (for as such we think it fully entitled to rank *) for which it is necessary to seek a cause, and for which no other rational explanation seems to offer."

But under the severity of recent experiment, besides the temperature having been supposed to be perceptibly raised in a condensing mirror of three feet diameter by the Italians, my son Piazzi, in his late experimental trip to Teneriffe, by the most delicate physical measurements conceivable, found that the heating influence of the moon's rays amounted to rather less than half that of a wax candle placed at a distance of 15 feet from him. Hence it is demonstrated that she reflects solar heat, and, though almost in a vanishing trace, still sufficient to warrant some of her imputed influences.

So much for the heat of the moon; and, regarding her light, it is found to be defective in all the specific rays which are wanting also in solar light; moreover it is polarized in planes conformable to the laws of polarization from rough surfaces. Still, with respect to the *questiones vexatæ* of a lunar atmosphere and of the *vis motrix* of the occultation phenomena (Cycle, I. pages 133 to 135), we are precisely where we were. On this head, a scientific reader of my remarks was so persuaded that the stars seeming to run in upon the face of

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* "From my own observation, made quite independently of any knowledge of such a tendency having been observed by others. Humboldt, however, in his personal narrative, speaks of it as well known to the pilots and seamen of Spanish America." (Herschel.) To this may be added our own old sea-adages, that *The full moon brings fine weather*, and that *Full moons eat up clouds*.

the moon, was obviously owing to the aberration of light, that a sort of altercation ensued. Failing to enlighten him myself, I appealed to the arbitration of the Astronomer Royal, whose answer to me (3rd August, 1846) was I think sufficiently conclusive for my positive but well-meaning friend:—

I conceive that no theory of aberration can give the slightest assistance towards the explanation of the projection of stars on the moon.

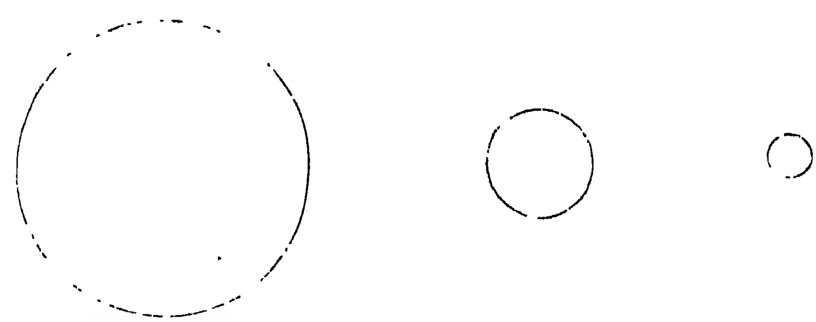
Whatever be the proper motions of the star, the moon, or the earth, the wave which comes from the star past any point of the moon, will come to the eye just as if it originated from that point of the moon, and not as if it came from an interior point.

But we must not take leave of the sun and moon without noticing that their beams have, under modern drilling, proved to contain something more than light and heat; for there is a positive chemical power that is altogether distinct from the more obvious properties of solar influence, and which has called the beautiful art of heliography, or sun-drawing, into existence. This astonishing effect of the solar beams passing by refraction through a lens on paper rendered exquisitely sensitive, whether by caustics, metallic solutions, or otherwise, is well known; and now it is equally evident that the moon receives and reflects this peculiar quality, as well as the light and heat of the sun, though with greatly diminished force—moonlight being but diluted sunlight. Hence PHOTOGRAPHY must be regarded as one of the most striking wonders of modern science, yielding portraits of whatever subjects with a boldness of shadow and precision of delineation beyond artistic skill, and on whatever scale may be desired, down to the minutest, requiring even microscopic aid to render them visible. Yet this result is produced instantaneously, in a few seconds, or at most in a few minutes, according to circumstances: and it is only in progress!

MARS (Cycle, I. page 148). In consequence of Captain Jacob's writing from the Madras Observatory, that in 1857 the greenish tint, which some observers have remarked on certain spots of Mars, was not perceived by him, I scrutinized its disc under favourable circumstances. Again have I to confirm the Herschelian assertion as to the colour of the continents being a dull or brownish red; that

of the seas greenish—perhaps by contrast; and the poles, especially the southern one, white. But the Captain also says, that the general hue appeared to him similar to the general appearance of the Earth's shadow on the moon, which is also considered a variety of green; and that shadow, when not of a palpable coppery tinge, always struck him as being of a neutral brown or dun tinge. This might be owing to an affection of the eye, to the medium looked through, or to both; such differences however are worthy of note in our present irregular chromatism. Captain Jacob's telescope was by Lerebours, 6·25 inches aperture, charged with a power of 277; and I used the Hartwell refractor of $5\frac{9}{10}$ inches, under a negative eye-piece magnifying 240 times.

In speaking of the variability of Mars (Cycle, I. pages. 150-1), of course I was alluding to the surprising changes of brightness consequent upon, and corresponding with, the great variations of his distance. Now, though a practised observer may be well acquainted with the differences of the mean diameter of this and the other erratic bodies of our system, as compared with their apparent diameters at conjunction and opposition, yet a diagram representing the proportionate or relative apparent magnitudes of Mars, at his extreme distances and at his mean distance from the earth, may serve at once to remind the practised and to guide the young amateur:



Mars at opposition or least distance. At mean distance. At conjunction or greatest distance.

The Asteroids (Cycle, I. pp. 152-161). In the year 1844 I closed my account of these bodies with the elements and particulars of Ceres, Pallas, Juno, and Vesta; after the discovery of which, and the discontinuance of Olbers's

search, years had rolled on without further thought on the subject, except what was awakened by Cacciatore's letter to me from Palermo in 1835 (Cycle, II. page 264), in which he lamented that a planet of some kind or other had slipped through his fingers. But towards the end of 1845, the astronomical community were aroused by an announcement from M. Hencke, of Driessen in Prussia, that, after having spent the leisure of 15 years perseveringly in the search, he had discovered another of the extra-zodiacal minors, and named it Astræa. It was found to revolve round the sun at nearly the same distance as the others, in a plane almost passing through the same nodes, and accomplishing its revolutions in 4 years and 52 days; which differs very little from the hypothetic prediction of my distinguished friend the late Baron de Zach. On the 9th of January 1846, when this notice was given to the Royal Astronomical Society, I happened to be in the chair, and therefore witnessed the animated pleasure—not unmingled with surprise—with which it was received by the Meeting: and the remarks made that evening on the discovery will be found in the Society's Monthly Notices (vol. vii. pages 27, &c.). Scarcely had this news cooled, ere M. Hencke picked up another fragment, which he dubbed Hebe, with a sidereal period of three years and 285 days; and it befel me to be again presiding at this second announcement, when I heard, in the course of the evening, the fixed determination of more than one friend to seek for additional splinters of the supposed shattered planet.

This unlooked-for intelligence was the signal of a new era in astronomical research, since, the alarm being given, it struck upon the ears of men who were willing, energetic, and ripe for systematic operations in the ecliptical regions of the sky. Besides the professional regulars of the astronomical corps, the volunteer amateurs—a body unconnected with any national establishment—advanced and did good work; so much so indeed as to reap one-third of the unexampled harvest that followed. And mark the supply of improved practice which ensued on the demand: when Ceres was first singled out, her rating as a planet was regarded with grave suspicion, and astronomers were by no means satisfied as to her claims in that respect for a considerable time: but

now a gazer would make up his mind in thirty minutes. Even in the instance of Iris, the first planetoid which fell to the lot of England to catch up—little more than an hour sufficed to mark her one of the *errantes*, as is well shown by a letter which Mr. Hind wrote to me on the 14th of August 1847:—

At half past 9 o'clock last evening I discovered a new planet near the star Piazzi xix, 396. It evidently belongs to the group between Mars and Jupiter, and may possibly prove to be the object seen and afterwards so unaccountably lost by Cacciatore in 1835. This however is mere supposition. It shines as a star of 8·9 magnitude, and the observed positions are:—

G. M. T.			Æ of Planet.				δ of Planet.		
August 13, 9^h	35^m	17^s	19^h	57^m	$30{\cdot}52^s$	—	13°	27′	23·4″
,, 10	45	19	19	57	28·02	—	13	27	29·0

Whence the diurnal retrograde motion in Æ amounts to 51^s: the change of declination being very small.

Such was the effect of the vigorous perseverance aroused in scouring the zodiacal latitudes, that we have now no fewer than 55 of those bodies amenable to law:* and it is with singular pleasure I record that eleven were discovered in the observatory which was devised and equipped on my plan and counsel, by my friend Mr. George Bishop, in Regent's Park. The finding and winning of these physically interesting, but otherwise apparently useless, spheres, were mostly communicated to me by the promptest means; and I have especially to thank Messrs. Hind, De Gasparis, Chacornac, Pogson, and my American friends Bond, Everett, and Maury, for their ready and early notifications. As to the first of these gentlemen, his appetite for the quarry was voracious in the extreme, since he desisted not till he had captured ten.

On the 14th of May 1854, I had the honour of proposing Mr. Hind to the Council of the Royal Society for their royal medal; on which occasion I was able to support the claim in these unusual terms:—

Mr. J. R. Hind has discovered no fewer than ten new planets, and computed the elements of their orbits—first from his own observations, and again from those of other astronomers: and he has greatly improved our knowledge of the motions of all the members of the Planetoidal group, by similar discussions, in each case, of all available data.

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* It was only in 1689 that Martin Szent-Juarry, in his *Curiosiora et selectiora Variarum Scientiarum*, says—"Omnes quinque minores Planetæ, nempe: Saturnus, Jupiter, Venus, Mars, et Mercurius, sunt," &c.

Mr. Hind has also discovered two new comets, and tended greatly to procure multiplied observations of them, and of others, by his rapid calculation and speedy publication of successive approximations to the elements of their orbits, having thereby enabled many comets to be followed up through important portions of their paths, which would otherwise have been lost—as witness the momentous and then unique case of his comet of 1847, which was observed at its perihelion passage at noon-day, and in the immediate proximity of the Sun, solely in consequence of the accuracy of Mr. Hind's computed places.

He has moreover discovered an elliptical nebula, a new deeply-coloured star, and several variable stars, including the remarkable changes in S Cancri, of which he published an ephemeris. He has strengthened the evidence of a physical connexion between the constituents of binary stars; and he has accurately mapped and published, for the advantage of astronomers in general, all the stars in a large part of the ecliptic region of the sky, down to and including the 11th magnitude. These maps cannot fail to be of great utility in promoting the future discovery of planets and asteroids ; in fact, of gleaning the heavens in that very interesting department."

There has been much coyness among savans as to the nature and condition of this suggestive group of fragments; and perhaps we must rest on our oars for some time yet before opinions are in unison. But from their exceptional minuteness of bulk, their mean solar distances, and their near orbital approximation at the points of mutual intersection—yet differing very widely from each other in their obliquity, excentricity, and inclination—the Olbersian hypothesis is really countenanced (Cycle, I. page 157). Still, although that theory receives fresh accessions of probability in the discovery of every new asteroid, it would be premature as yet to regard it as one of the demonstrated truths of science. Their magnitudes are too small for precise measurement by any of our present methods, but from free estimation we may assign to the generality of them a diameter of 100 miles $\pm$: and this is a liberal allowance,* since M. Leverrier finds that their aggregate mass must be less than one-fourth that of the Earth, because, if equal to that quantity, it would, in course of time, have had such a sensible effect on the solar position of Mars, as could not have escaped the notice of astronomers.

Then look at their cruizing-ground. The perihelion distance varies between 1·798 of the Earth's mean distance from the Sun, or about 171 millions of miles, for Melpomene, and 2·853, or 272 millions, for Hygeia; while their greatest solar distances are included between the aphelion of Flora, 243 mil-

* According to Argelander, the mean diameter of the first 39 asteroids = 72·6 English miles.

lions of miles, and 335 millions for Themis; and their period of revolution about our central orb, between that of Flora, or 1193 days, and 2054 for Hygeia. Thus the first of these splinters is the nearest to the sun, and the second the most distant. Atalanta is the faintest of the group, and Hestia the next, being respectively of 12·9 and 12·5 magnitudes when in opposition, the planets and our earth being supposed at their mean solar distance.

But the windings of their respective paths must long form a most difficult tangle for the human mind to extricate, since, unlike the older planets with ellipses and inclinations geometrically moderate, the series expressing the co-ordinates of these small bodies become extremely complicated and unconvergent. Moreover we know next to nothing respecting the retarding influence of an ethereal medium (Cycle, I. pages 251 and 252), but which, notwithstanding the terrors of the Vacuumites, really seems to exist. There are still some—aye, and A1 men too—who set themselves dead against this conviction, though in the present state of the argument they mostly carry fuller canvass than ballast. The labyrinth gets more and more intricate as we dive into the elements of the asteroids. The variety in their orbital inclinations—as for instance Pallas 35° and Massilia not 1°—may have arisen from the original impulse at the moment either of internal explosion, or external shock, and their mutual disturbances while they were yet so near each other that their forms had an influence on their attractive force. Even now their orbits may be subject to great variations, for some of them are such close neighbours—Iris and Vesta to wit—that their reciprocal perturbations may probably invert their mean distances from the sun. To the Poet's eye, these planetoids may seem to be "dancing the hays" as they sail along through the fathomless immensity of space in intertangled courses, though from their diminutive size no other eye sees them unassisted: while, from the smallness of their masses, the force of gravity on the surface of their bodies must be so inconsiderable, says Herschel, that "a man placed on one of them would spring with ease sixty feet high, and sustain no greater shock in his descent than he does on earth from leaping a yard;" and on some of them the astronomer would find a length of two inches quite sufficient for his seconds pendulum.

Some very angry expletives have been aimed at the nomenclature, but it is rather firing off wadding than shot; and, from what a friend tells me of the example of using modern names, as adopted by Beer and Mädler in their Selenography, it may be apprehended that it would not be the votaries of science who would find the greatest favour (see Cycle, vol. I. pages 436 and 437).* Happily, the symbols at first bestowed on these diminutive bodies are for ever abolished, and Dr. Gould's encircled numbers universally adopted.

Who could have ventured to predict half a century ago, that, before the year 1860, one large planet, more than half a hundred small ones, and a batch of periodical comets, would be added to the Solar system as known in 1800? Yet, we repeat, except their geometrical positions, very little is known about them; but, in order to arrive at their orbits with greater accuracy, the astronomers of various countries have organized a division of labour which is eminently calculated to harness the asteroids. This has been carried by a concerted agreement among themselves, in assigning the task of closely attending an allotted portion of them to particular public and efficient observatories; each astronomer taking charge of, and making himself accountable for, exact determinations of some five to eight members of this atomic family. In October, 1856, the accepted distribution stood thus:—

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* When requested to name (46), I was desirous of calling it Horus, the son of Isis, the latter having also been discovered at Oxford, near the banks of the Isis, by Mr. Norman Pogson; but, finding the nomenclature restricted to females, I suggested Hestia. This has been thought to borrow too closely on Vesta; but, though the latter was the Roman copy of the former, the Greek worship of Hestia differed in several points. A stern moralist might demur as to the character of some of the ladies who have been thus placed aloft, and who could rebut him? While on names, the objection made in America to Hind's *Victoria* should be mentioned, since it appears to have been forgotten that our Sovereign bears the name of one of the olden deities; but the objection was hardly taken, ere I received the following note on the subject from the Hon. Edward Everett, which I forthwith communicated to the Royal Astronomical Society (Monthly Notices, xi. 27):—"I perceive that Mr. Hind has received the impression from Mr. Gould (editor of the Astronomical Journal), that the American astronomers were not disposed to acquiesce in the name of *Victoria*, proposed by him for the last asteroid. When that name was first proposed, it was considered by some persons to be inconsistent with the principle which had been agreed upon in reference to the designation of newly-discovered bodies. But, as Mr. Hind states that he regards *Victoria*, in this connexion, as the name of a mythological personage, I cannot think that its coincidence with that of your Sovereign ought to forbid its use. Mr. Bond fully agrees with me in this opinion, and there is no one in this country of higher authority than he on any astronomical question."

No.			No.		
1	Ceres	Copenhagen, Rome.	22	Calliope	Göttingen, Vienna, Ann Arbor.
2	Pallas	Copenhagen, Rome.	23	Thalia	Berlin, Vienna.
3	Juno	Copenhagen, Rome.	24	Themis	Cambridge (*Eng.*), Berlin.
4	Vesta	Albany, Rome.	25	Phocæa	Washington, Berlin.
5	Astræa	Göttingen, Vienna, Ann Arbor.	26	Proserpine	Cambridge (*Eng.*), Ann Arbor.
6	Hebe	Altona, Ann Arbor, Bilk.	27	Euterpe	Vienna.
7	Iris	Altona, Albany.	28	Bellona	Cambridge (*Eng.*)
8	Flora	Cambridge (*Eng.*), Ann Arbor.	29	Amphitrite	Altona, Albany.
9	Metis	Cambridge (*Eng.*), Ann Arbor.	30	Urania	Cambridge (*Eng.*)
10	Hygeia	Altona.	31	Euphrosyne	Berlin, Vienna, Ann Arbor
11	Parthenope	Altona, Bilk.	32	Pomona	Vienna.
12	Victoria	Cambridge (*Eng.*), Ann Arbor.	33	Polyhymnia	Berlin, Vienna.
13	Egeria	Washington.	34	Circe	Cambridge (*Eng.*), Berlin.
14	Irene	Washington.	35	Leucothea	Washington, Cambridge (*Eng.*)
15	Eunomia	Altona, Albany.	36	Atalanta	Berlin, Vienna.
16	Psyche	Washington, Berlin.	37	Fides	Washington.
17	Thetis	Washington.	38	Leda	Göttingen, Vienna.
18	Melpomene	Washington.	39	Lætitia	Altona.
19	Fortuna	Altona.	40	Harmonia	Vienna.
20	Massilia	Göttingen, Vienna.	41	Daphne	Vienna.
21	Lutetia	Berlin, Vienna.	42	Isis	Göttingen, Vienna

May the daughter of the great Zeus, the celestial *Οὐρανια* herself, speed the labours of this *ἱερα φαλαγξ;* and, when we shall have attained a greater amount of certainty respecting the Asteroids, a close study of their orbits will tend to show whether the hypothesis of a large planet's explosion be tenable or not. Although they have no doubt originally been thus created for wise and inscrutable purposes, it cannot be denied that all the recent discoveries strongly countenance the idea of a tremendous cataclysm having occurred, however little such an event may appear to be in harmony with cosmogony. For my own part as to these bodies, so minute, yet as facts so essential to all future speculations on the solar system, I have only occasionally looked at them when their several discoveries were from time to time announced. Under coaxing management in the telescope, the largest of them present a steady, pale disc, contrasting with the brighter twinkle of neighbouring stars of apparently similar magnitude; but, as they are off my beat, and had but little of my attention, I shall here insert their elements as gathered together for me by Mr. Norman Pogson, precursing with his remarks to me on their tabulation, dated from South Parade, Oxford, July 22nd, 1858:—

As early as possible I set to work to prepare the required table; but, as you kindly left me much liberty as to its arrangement, I have drawn it up exactly as I know by my own frequent wants is most convenient for reference. I have long intended to compile such a table for my own private use, but other matters have hitherto prevented me.

You will observe that this table is on two pages, of which the first bears the historical details, and the second the elements. I will remark that, in the fourth column of the first page, by adding the word Observatory a just distinction is drawn between amateurs who devote their time as a labour of scientific love, and those who are paid for their work, and only successfully fulfil their duty. I am the only loser by such proposal in the cases of Isis and Ariadne; but then my right to Hestia is completely established. My magnitudes on column 6 are to be inserted in the Nautical Almanack for 1862; they are identical with Argelander's, and therefore the *mean opposition magnitude*, calculated on the generally employed ratio of 2,512, becomes an additional element of the orbit. The seventh column has been fresh computed, and in the eighth the names of the painstaking and praiseworthy computers are enrolled.

We now come to the element page—in the first column of which the epochs are all expressed in Greenwich mean time, and the frequent recurrence of the fraction ·96279 is in consequence of most of the elements having been computed for Berlin noon. If preferable, it would be very little trouble to alter the mean anomalies, so as to dispense with fractions of a day entirely, and, for Greenwich mean *time*, put Greenwich mean *noon*. In the remaining columns all the elements have been collected from the latest, and apparently the best, sources. When the π and ☊ are referred to another equinox than the epoch, I have corrected them to the date given in column 1 of this page. I cannot but plead against the expression *Longitude of Perihelion*, counted as it is partly on the ecliptic and partly on the orbit; the single word is far better. In the last column the Log. Mean Distance is much more useful than the natural numbers. Mean daily sidereal motion is so easily obtained if desired, that it does not merit a separate column, though, if there were one to spare, its logarithm would be a convenient addition.

In reference to this formidable array of little planets I may merely mention that Juno was found with the smallest telescope ever used for such a purpose,—only two inches in aperture. Irene was discovered independently by De Gasparis just four days later than by Mr. Hind; but the finding of Psyche was De Gasparis's reprisal for Irene, he therein forestalling Hind, though only by one day. Massilia was found independently by Chacornac the night after its discovery at Naples; and in like manner Amphitrite was picked up by myself one night only after Marth had secured it. Virginia was announced as discovered by Luther on the 19th of October, but Ferguson had preceded him in Washington by about fifteen days.

Vesta, when favourably situated, is distinctly visible to the naked eye, and has been so seen by me; it can become much brighter than Uranus. Small planets may exceed their mean opposition-magnitudes by two magnitudes, or more than six times their whole quantity of light, *on our ratio;* or, if the Earth be in perihelion when the planet's opposition occurs near its aphelion, it may fall as much short of the mean value; but I firmly believe that all the fancies about their variability are mere imagination, though I once indulged in a similar belief about Iris.

Hestia is the faintest but one (Atalanta) of the small planets, and Argelander was astonished that I could observe it with the Smythian telescope, which has only 3·6 inches of clear aperture; in most oppositions it will be absolutely invisible with that instrument, excellent as it is

HISTORICAL TABLE OF THE FIFTY-FIVE SMALL PLANETS.

Name.	Numerical Symbol.	Discovered			Mean Opposition Magnitude.	Sidereal Period in Days.	Calculator of Elements.
		By	At	On			
						d.	
Ceres . .	1	Piazzi . .	Palermo Observatory .	1801 Jan. 1	7.7	1680.2	Schubert
Pallas . .	2	Olbers . .	Bremen	1802 March 28	7.9	1683.7	Farley
Juno . .	3	Harding .	Lilienthal	1804 Sep. 1	8.7	1593.1	Farley
Vesta . .	4	Olbers . .	Bremen	1807 March 29	6.6	1324.8	Farley
Astrea . .	5	Hencke . .	Driessen	1845 Dec. 8	10.0	1511.7	D'Arrest
Hebe . .	6	Hencke . .	Driessen	1847 July 1	8.6	1380.2	Luther
Iris . . .	7	Hind . .	South Villa Observatory	1847 Aug. 13	8.6	1346.4	Schubert
Flora . .	8	Hind . .	South Villa Observatory	1847 Oct. 18	8.9	1193.0	Brünnow
Metis . .	9	Graham .	Markree Observatory .	1848 April 25	8.9	1345.9	Wolfers
Hygeia .	10	De Gasparis	Naples Observatory . .	1849 April 12	9.8	2053.5	Zech
Parthenope	11	De Gasparis	Naples Observatory . .	1850 May 11	9.5	1402.9	Luther
Victoria .	12	Hind	South Villa Observatory	1850 Sep. 13	9.6	1302.8	Brünnow
Egeria . .	13	De Gasparis	Naples Observatory . .	1850 Nov. 2	9.9	1509.8	Günther
Irene . .	14	Hind . .	South Villa Observatory	1851 May 19	9.7	1521.9	Bruhns
Eunomia .	15	De Gasparis	Naples Observatory . .	1851 July 29	9.1	1570.5	Trettenero
Psyche .	16	De Gasparis	Naples Observatory . .	1852 March 17	10.1	1825.2	Klinkerfues
Thetis . .	17	Luther . .	Bilk Observatory . .	1852 April 17	9.9	1422.7	Schoenfeld
Melpomene	18	Hind . .	South Villa Observatory	1852 June 24	9.5	1270.5	Bruhns
Fortuna .	19	Hind . .	South Villa Observatory	1852 Aug. 22	9.7	1393.3	Powalky
Massilia .	20	De Gasparis	Naples Observatory . .	1852 Sep. 19	9.3	1365.9	Günther
Lutetia .	21	Goldschmidt	Paris	1852 Nov. 15	10.3	1387.6	Lesser
Calliope .	22	Hind . .	South Villa Observatory	1852 Nov. 16	10.6	1812.7	Hornstein
Thalia . .	23	Hind . .	South Villa Observatory	1852 Dec. 15	11.0	1556.5	Schubert
Themis .	24	De Gasparis	Naples Observatory . .	1853 April 5	11.6	2036.6	Krüger
Phocea .	25	Chacornac	Marseilles Observatory .	1853 April 7	10.5	1360.0	Günther
Proserpina	26	Luther . .	Bilk Observatory . .	1853 May 5	10.7	1580.3	Oudemans
Euterpe .	27	Hind . .	South Villa Observatory	1853 Nov. 8	9.9	1313.5	Günther
Bellona .	28	Luther . .	Bilk Observatory . .	1854 March 1	9.8	1688.7	Bruhns
Amphitrite	29	Marth . .	South Villa Observatory	1854 March 1	9.1	1491.5	Günther
Urania .	30	Hind . .	South Villa Observatory	1854 July 22	10.1	1327.8	Günther
Euphrosyne	31	Ferguson .	Washington Observatory	1854 Sep. 1	11.6	2048.0	Winnecke
Pomona .	32	Goldschmidt	Paris	1854 Oct. 26	11.0	1519.5	Lesser
Polyhymnia	33	Chacornac .	Paris Observatory . .	1854 Oct. 28	11.4	1770.9	Pape
Circe . .	34	Chacornac .	Paris Observatory . .	1855 April 6	11.5	1610.5	Anvors
Leucothea	35	Luther . .	Bilk Observatory . . .	1855 April 19	12.1	1873.1	Rümker
Atalanta .	36	Goldschmidt	Paris	1855 Oct. 5	12.9	1664.5	Förster
Fides . .	37	Luther . .	Bilk Observatory . . .	1855 Oct. 5	10.5	1568.5	Rümker
Leda . .	38	Chacornac .	Paris Observatory . .	1856 Jan. 12	10.9	1656.5	Allé
Lætitia .	39	Chacornac .	Paris Observatory . .	1856 Feb. 8	9.3	1684.8	Allé
Harmonia	40	Goldschmidt	Paris	1856 March 31	9.1	1246.8	Powalky
Daphne .	41	Goldschmidt	Paris	18. 6 May 22	10.2	1358.3	Pape
Isis . .	42	Pogson . .	Radcliffe Observatory .	1856 May 23	10.6	1386.9	Seeling
Ariadne .	43	Pogson . .	Radcliffe Observatory .	1857 April 15	10.0	1195.0	Weiss
Nysa . .	44	Goldschmidt	Paris	1857 May 27	10.4	1383.9	Gussew
Eugenia .	45	Goldschmidt	Paris	1857 June 27	11.0	1659.1	Löwy
Hestia . .	46	Pogson . .	Oxford	1857 Aug. 16	12.4	1477.9	Watson
Aglaia . .	47	Luther . .	Bilk Observatory . . .	1857 Sep. 15	11.5	1781.1	Oeltzen
Doris . .	48	Goldschmidt	Paris	1857 Sep. 19	11.4	2000.1	Powalky
Pales . .	49	Goldschmidt	Paris	1857 Sep. 19	10.9	1980.2	Powalky
Virginia .	50	Ferguson .	Washington Observatory	1857 Oct. 4	12.3	1574.6	Stockwell
Nemausa .	51	Laurent . .	Marseilles Observatory .	1858 Jan. 22	10.4	1339.4	Förster
Europa .	52	Goldschmidt	Paris	1858 Feb. 6	10.7	1993.4	Murmann
Calypso .	53	Luther . .	Bilk Observatory . . .	1858 April 4	11.5	1542.7	Oeltzen
Alexandra	54	Goldschmidt	Paris	1858 Sep. 10	11.2	1642.4	Schjellerup
Pandora .	55	Searle . .	Dudley Observatory . .	1858 Sep. 10	11.1	1674.3	Möller

ELEMENTS OF THE ORBITS OF THE FIFTY-FIVE SMALL PLANETS.

Epoch of Elements. Greenwich Mean Time.	M Mean Anomaly.	ϖ Perihelion.	ν or Ω Ascending Node	ϕ Excentricity.	i Inclination.	log a log Mean Distance
	° ′ ″	° ′ ″	° ′ ″	° ′ ″	° ′ ″	
1859 Sep. 9. 00000	197 45 44.0	140 25 29.6	80 49 50.1	4 36 19.7	10 36 30.9	0.441862
1859 Aug. 4. 00000	194 38 52.2	122 9 37.5	172 38 15.5	13 51 47.5	34 42 44.8	0.442481
1859 April 30. 00000	153 2 4.0	54 6 6.0	170 59 7.7	14 49 38.4	13 2 58.8	0.426446
1859 Oct. 3. 00000	111 29 15.9	250 28 5.9	103 22 12.3	5 10 21.3	7 8 10 8	0.373052
1859 March 1. 00000	24 37 46.3	135 5 40.3	141 26 15.7	10 44 11.0	5 19 8.1	0.411245
1858 May 7. 96279	226 53 44.3	15 7 48.9	138 35 28.8	11 37 7.7	14 40 24.1	0.384901
1860 Feb. 8. 96279	73 29 43.0	41 29 40.8	259 47 16.1	13 22 13.1	5 27 57.4	0.377734
1847 Dec. 31. 96279	35 54 3.6	32 54 28.3	110 17 48.6	9 0 56.3	5 53 8.0	0.342696
1858 June 20. 96279	57 4 17.1	71 3 55.6	68 31 31.6	7 5 1.6	5 36 0.6	0.377621
1858 April 11. 00000	185 20 23.4	231 45 1.2	286 53 1.7	5 36 41.7	3 47 26.3	0.499948
1858 June 26. 96279	327 46 34.8	316 10 7.1	125 3 41.1	5 40 30.3	4 36 57.9	0.389625
1859 Jan. 24. 00000	159 57 51.3	301 50 33.7	235 37 17.3	12 41 41.0	8 23 12.4	0.368202
1858 Sep. 25. 96279	251 52 56.5	119 31 16.9	43 19 29.5	5 2 3.2	16 32 24.2	0.410870
1857 Nov. 19. 96279	247 43 58.7	179 28 21.9	86 40 4.5	9 30 38.1	9 7 7.4	0.413194
1859 June 17. 00000	219 47 15.6	27 32 11.0	293 56 37.0	10 47 56.8	11 43 37.8	0.422306
1855 Nov. 25. 96279	38 54 50.5	12 40 34.3	150 32 48.5	7 44 14.8	3 4 7.6	0.465809
1858 April 11. 00000	130 48 12.9	259 57 18.0	125 25 7.3	7 15 50.4	5 35 22.0	0.393686
1860 Jan. 19. 00000	346 12 56.8	15 13 15.4	150 4 16.7	12 31 55.0	10 8 38 4	0.360928
1858 March 6. 00000	118 53 20.0	30 23 30.5	211 25 0.2	9 5 14.3	1 32 33.7	0.387626
1859 Aug. 21. 96279	225 15 30.9	98 36 34.5	206 42 29.2	8 16 11.1	0 41 7.3	0.381892
1860 Jan. 19. 00000	22 54 32.4	326 27 0.6	80 30 56.2	9 18 22.0	3 5 21.0	0.386454
1859 Dec. 30. 96279	168 12 13.5	56 34 13.1	66 36 21.8	5 51 7.4	13 45 28.4	0.463818
1860 Jan. 19. 00000	200 43 22.4	123 59 0.5	67 38 36.4	13 24 11.4	10 13 5.7	0.419712
1859 April 6. 00000	54 0 28.6	139 6 1.5	36 10 6.6	6 42 5.7	0 48 55.8	0.497551
1858 Dec. 22. 96279	132 24 12.0	302 54 40.7	214 4 15.4	14 40 33.0	21 34 53.6	0.380630
1860 Feb. 6. 00000	186 24 6.1	234 50 23.9	45 55 3.4	5 0 0.3	3 35 38.0	0.424099
1859 June 13. 96279	173 4 32.7	87 30 0.0	93 44 45.0	9 57 22.5	1 35 31.1	0.370568
1861 Feb. 27. 96279	36 40 48.5	122 22 48.3	144 43 5.4	8 53 17 5	9 22 30.8	0.443291
1859 July 8. 96279	236 32 17.2	56 30 6.6	356 26 51.8	4 9 3.1	6 7 49.6	0.407369
1858 Oct. 8. 96279	348 6 50.7	31 23 24.7	308 13 46.3	7 18 22.7	2 5 56.9	0.373684
1854 Dec. 30. 96279	310 58 43.7	93 51 6.6	31 25 23.0	12 28 29.8	26 25 12.4	0.499159
1858 Oct. 8. 00000	188 52 32.2	193 44 58.4	220 50 36.2	4 41 52.9	5 29 3.9	0.412752
1854 Dec. 30. 96279	42 23 52.5	340 41 55.8	9 14 30.4	19 44 7.9	1 56 48.0	0.457066
1856 July 12. 96279	146 32 31.2	150 3 51.9	184 44 56.6	6 8 42.4	6 26 36.8	0.429588
1855 April 29. 96270	359 32 30.3	108 17 0.2	356 24 37.9	12 30 13.5	8 15 17.7	0.473296
1855 Dec. 30. 96279	353 57 28.2	42 22 25.0	359 8 48.4	17 19 53.4	18 42 9.5	0.439128
1855 Dec. 30. 96279	336 28 54.5	66 5 35.8	8 10 23.4	10 4 0.8	3 7 19.3	0.421918
1855 Dec. 30. 96279	12 11 49.1	100 44 30.7	296 27 34.9	8 56 50.2	6 58 26.3	0.437747
1855 Dec. 31. 96279	144 36 37.9	2 7 12.4	157 19 38.9	6 21 43.8	10 21 0.4	0.442645
1856 Dec. 30. 96270	273 3 15.0	2 2 16.0	93 32 28.0	2 38 29.0	4 15 48.4	0.355229
1856 May 31. 4628	332 7 19.	230 21 30.	180 5 51.	11 40 57.	15 48 23.	0 33027
1856 June 30. 96279	318 47 13.5	317 57 34.4	84 27 49.7	12 52 50.1	8 34 39.6	0.336801
1857 April 16. 96279	306 51 0.9	277 14 9.5	264 29 27.4	9 38 46.6	3 27 47 6	0.343180
1857 July 9. 96279	121 9 11.4	111 46 11.9	130 54 33.1	8 25 51.6	3 41 56.6	0.385675
1856 Dec. 30. 96279	340 24 33.9	235 4 34.4	147 51 37.8	4 52 10.7	6 35 59.1	0.438109
1857 Sep. 19. 71398	338 1 57.7	354 55 42.7	181 24 50.4	9 48 15.3	2 17 51.0	0.404705
1857 Nov. 28. 30860	50 19 45.8	312 9 19.3	4 28 35.0	7 32 58.4	5 0 26.6	0.458732
1857 Oct. 30. 96279	281 51 49.6	77 11 47.5	185 13 39.7	4 25 19.8	6 29 44.0	0.492320
1857 Oct. 30. 96279	337 40 5.6	32 49 23 3	290 27 1.0	13 44 54.4	3 8 25.0	0.480412
1859 March 1. 00000	118 44 58.3	10 12 46.9	173 30 0.0	16 41 4.1	2 47 45.0	0.423066
1858 March 2. 55279	342 34 21.8	190 12 40.2	175 37 44.1	3 36 13.0	10 14 39.4	0.376196
1857 Dec. 30. 96279	31 11 34.8	102 14 26.1	129 56 57.2	5 47 35 6	7 24 34.9	0.491345
1858 April 27. 96279	75 27 38.5	94 38 52.4	143 30 27.9	10 23 3.6	5 3 38.8	0.417122
1858 Sep. 25. 31812	23 5 34.4	306 19 28.0	313 22 43.9	10 50 23 7	11 31 21.0	0.435260
1858 Nov. 4. 46279	5 7 34.2	11 21 34.5	10 57 30.3	8 9 44.0	7 13 31.6	0.440835

Jupiter (Cycle, I. pp. 167 to 184.) The volume here cited had hardly been launched, before it was found that on page 168 (line 10 *ab imo*) the printer had mistaken a 3 for an 8; and, altering the figure into a word, the error was overlooked on the proof-sheet. This, however, would not probably lead to any blunder, for it was obvious to the "meanest capacity;" and moreover the ratio of surface gravities is shown on the bottom line of the same page.

The stupendous disc of this large planet is a truly interesting object, and still demands closer attention; but, not being upon my regular working agenda, it has only been occasionally viewed by me. Although its light has neither the brilliance nor the whiteness of that of Venus or Mercury, the mild lustre of its surface is at once pleasing and beautiful, which soon led to a critical examination of it after the invention of telescopes, and the consequent discovery of both bright and dark spots upon it (Cycle, I. p. 171.) Latterly, however, there had been neglect in the matter, on the common delusion perhaps that all had already been done which could be expected. Thus we dozed till the year 1849, when the Rev. W. R. Dawes perceived some very remarkable white dots on Jupiter, which he likened to the circular craters on the moon. On the 27th of March of the following year Mr. Lassell saw them in his 20-foot equatoreal reflector, and sent the sketch of these singular objects to the Astronomical Society, from which the engraving is made that appears in their Monthly Notices (vol. x. page 134). Again, in May 1850, Professor Schumacher, of Altona, observed four or five of these features on one of the belts, which he thus describes—"These white spots are most remarkable. They are all perfectly round, distinct, and bright. The largest of them is as distinct and well-defined as the disc of a satellite appears in a 9-foot reflector. They are striking phenomena, keeping their relative positions as they are carried along by Jupiter's rotation, and there are no other similar spots on his disc."

It so happened that, when this tocsin was sounded, I was not in observing action; and as, when operations re-commenced, my efforts were otherwise directed, the Jovian spots were not borne in mind. Assuredly such neglect ought not to occur where a fine equatoreal is in use, but it affords another

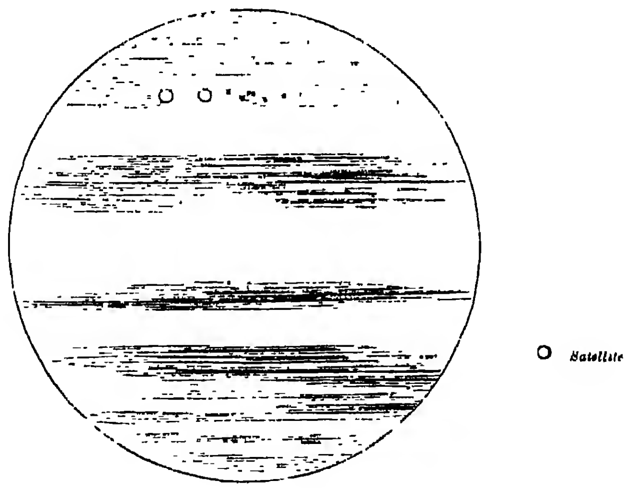
proof, if proof were wanting, of the necessity of a division of labour to meet cases even in astronomy. Matters remained in this state with me till the 13th of October, 1857, when I received a letter from Sir William Keith Murray, of Ochtertyre, informing me that while gazing upon Jupiter on the 6th of that month—but his own words will be best:—

Allow me to thank you for your note, and I shall not fail to look for the objects that you mention the first clear night. I have lately been testing Cooke's 9-inch object-glass on γ Andromedæ and other objects, and am glad to say it promises well. Of the colours of the first, A is orange, B pale yellow, and C smalt blue: there is no doubt about C, but B is very pale—neither red nor blue—a pale yellow, as well as my eye and apparatus as yet can determine. This may account for the blue-green colour in smaller instruments? B and C were beautifully clear, round, and sharp; indeed I was quite surprised with the perfection of definition. Powers 500 and 850.

On the 6th (October) at midnight, after being engaged with visitors for two hours, I turned the instrument on Jupiter to scrutinize the details of the belts, and immediately perceived two nearly equal, well-defined, white bright spots, followed by some irregular, minute white markings which defied my eye and instrument. At 1, I could no longer perceive them. I have again seen them, but too unfavourably to judge of any change. Powers used, 240 to 350. I shall not fail to watch them on every promising occasion.

The two larger spots were beautifully defined, and seemed about the same proportion, relative to size, as the disc of the first satellite. What is known of these spots? Can they be the ice and snow seen through openings in their clouds?

To this letter the scientific Baronet appended an illustrative pen-and-ink sketch of the surface of Jupiter, with the first satellite outside the disc as a scale, of which the following is a pretty faithful fac-simile:—



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A little time before this, Mr. Dawes had been observing these maculæ, and had also clearly made out five of them, as in the preceding diagram; on the 31st of October, he communicated the description and views which appear in the Astronomical Society's Monthly Notices (vol. xviii. pp. 6—8); which account was followed by further observations and drawings on the 10th of December. To these the reader is referred for the amount of information we are as yet in possession of, respecting these curious indications. My own hours were pre-occupied, insomuch that, though imbued with its physical import, I could only afford a very casual attention to the subject; but by pointing the telescope upon Jupiter, two or three times a gleamy mottled whiteness was caught on the equatorial belt. On the fine evening of the 4th of December, I saw a bright round spot on the planet's southern belt, under a magnifying power of 416; it was well defined, and so distinct that it had the semblance of a satellite transiting the disc. However, on lowering the power to 65, I found that all the satellites were outside the planet. Mr. Dawes appended a note of this observation to his above-cited letter.

A word more before quitting Jupiter. A very important suggestion has been thrown out by Sir John Herschel, which should be borne in view by those amateurs who would spend their time advantageously in determining the comparative intensities of light in the stars, so as by-and-by to enable us to obtain a better rating of their magnitudes—a most desirable end. For the advancement of such a purpose, Sir John recommends this magnificent body as a photometric standard of comparison:—"Such a standard (he says,) is offered by the planet Jupiter, which being much brighter than any star, subject to no phases, and variable in light only by the variation of its distance from the sun, and which moreover comes in succession above the horizon at a convenient altitude, simultaneously with all the fixed stars, and in the absence of the moon, twilight, and other disturbing causes (which fatally affect all observations of this nature), combines all the requisite conditions." Let our numerical valuers, sequence framers, and photometric star-meters look to this!

THE SATELLITES OF JUPITER (Cycle, I. page 175 to 176). In speaking of the visibility of these objects, I mentioned my want of success in gazing for them in the year 1814, with the unassisted eye, at the *Casa Inglese,* on the foot of the cone of Mount Etna, the door of which *casa* I ascertained to be 9,592 feet above the sea at Catania. I have now to add my son's trial of naked eye observations made on the Peak of Teneriffe, during his "Astronomical Experiment" in 1856. His two stations—*Estancia de los Ingleses*—were respectively settled by him as being severally 8,903 and 9,710 feet in height: "the stars shone brilliantly as seen from Guajara, and caused the dome of the skies to appear resplendent with glory; the Milky Way was a magnificent feature in its scenery, and the zodiacal light towards morning was still more remarkable. Jupiter also was surpassingly brilliant when high in the heavens after midnight; but I could never see his satellites with the naked eye, not even when eclipsing the planet behind a distant lava ridge."

SATURN (Cycle, I. pages 185—202). In opening the account of Saturn, and mentioning the dislike with which he was regarded by the ancients, it ought also to have been said, that all due attention had been paid to his march: and an observation of his being two digits below the star in Virgo's southern wing (γ *Virginis*), in March 228 B.C. is preserved in Ptolemy's Almagest. Since the volume above cited was printed, the enormous globe—the wondrous rings—and the attendant moons of the "plumbeous" planet have undergone an unusually rigid telescopic overhaul; by which Messrs. Struve, Dawes, Bond, Main, de Vico, Secchi, Lassell, Schmidt, and others, have brought various most interesting facts to light, notwithstanding its immense distance.

In examining the shape of the body, the parallelo-grammatic distortion assigned to it by the Herschels, and countenanced in a measure by Struve, is now imputed to some optical delusion, for the figure, according to the accurate micrometic measures of Professor Bessel and the Rev. Robert Main, at Könisberg and Greenwich, is proved to be all but a perfect ellipse, the equatorial diameter being to the polar as 1000 to 903. Again, astronomers have

evinced great solicitude for the Saturnians, regarding their dismally long winters. Sir John Herschel, in his second edition of *Outlines of Astronomy*, observed, relative to the ring, "in the regions beneath the dark side, a solar eclipse of fifteen years' duration under their shadows must afford (to our ideas) an inhospitable asylum to animated beings, ill compensated by the faint light of the satellites; but we shall do wrong to judge of the fitness or unfitness of their condition from what we see around us, when perhaps the very combinations which convey to our minds only images of horror, may be in reality theatres of the most striking displays of beneficent contrivance." This subject has been thoroughly ransacked by the late Dr. Dionysius Lardner, in a very elaborate essay which was read to the Astronomical Society in March 1853, and is published in the twenty-second volume of their Memoirs (pp. 40-102); in which he shews the effect of situation on the surface of the planet in modifying the phenomena presented by the rings. The results of the doctor's investigation do not confirm the nearly general opinion, that the rings hold an almost invariable position among the stars; and that consequently the long and dreary fifteen years of solar eclipse advanced, must be impossible.

The elder Herschel was the first to notice that the shading of the inner ring was not uniform, but gradually became darker towards the interior edge, giving the ring a convex appearance. In 1851 Dawes perceived a streakiness in the shading off, giving the semblance of a series of narrow, concentric bands, looking like steps leading down to the black chasm between the ring and the ball. Four such divisions were distinctly made out, and the impression he received was that they were separate rings, but too close together for the interstices to be seen as black lines.

Among the marvellous telescopic revealments of late years, is the discovery of the inner slate-coloured annulosity between the ball of Saturn and the bright ring, by Mr. Bond of the Cambridge Observatory in America, on the 11th of November, 1850. This being notified to me, I placed the document before the Royal Astronomical Society, where it was read on the 13th of December, 1850, and thus entered in their Monthly Notices (vol. xi. p. 20):—

We believe that no information has been received directly from Mr. Bond, but Captain Smyth has had a letter from the Hon. Edward Everett (dated 26th November last) from which the following extract is taken:—

"Mr. Bond has lately announced the discovery of a third ring of Saturn, interior to the other rings. This is not to be confounded with Struve's supposed resolution of the other rings, which Mr. Bond's observations do not confirm. The new ring is believed by Mr. Bond to be thicker than the other rings. Before he had ascertained its existence as a separate body, and while he thought it was a part of the inner one of the hitherto known rings, he supposed that the thickness of the latter was not uniform, but that it increased towards the planet. This appearance he now refers to the greater thickness of the edge of the newly discovered ring. He is entirely confident of the reality of the discovery; and, such are Mr. B.'s accuracy and caution, that I think it may fully be depended upon. He will before long communicate to the American Academy a full account of it."

This announcement excited great attention in America, and the appearance of increasing breadth of the new dark ring brought Mr. Bond to the conclusion that it cannot be of a solid consistency. Extending this conviction, he considered the discrepancies so often occurring, as to the visibility or otherwise in the annular divisions, which could not result from surface asperities, or coherence, and at once declared himself satisfied that the observed changes in form and position were in obedience to the laws of equilibrium appertaining to floating bodies. In this he was powerfully followed by Professor Pierce,* who demonstrated, from purely mechanical considerations, the possibility of the rings being streams of fluid, somewhat denser than water, flowing round the planet, and that the sustaining power for the support of these wondrous appendages exists in the satellites *exterior* to the ring system. Be this as it may, there are those who apprehend that if the said system does not exist, as hath been supposed, in a state of stable equipoise, we may expect sooner or later to see the rings unite with the body of the planet.

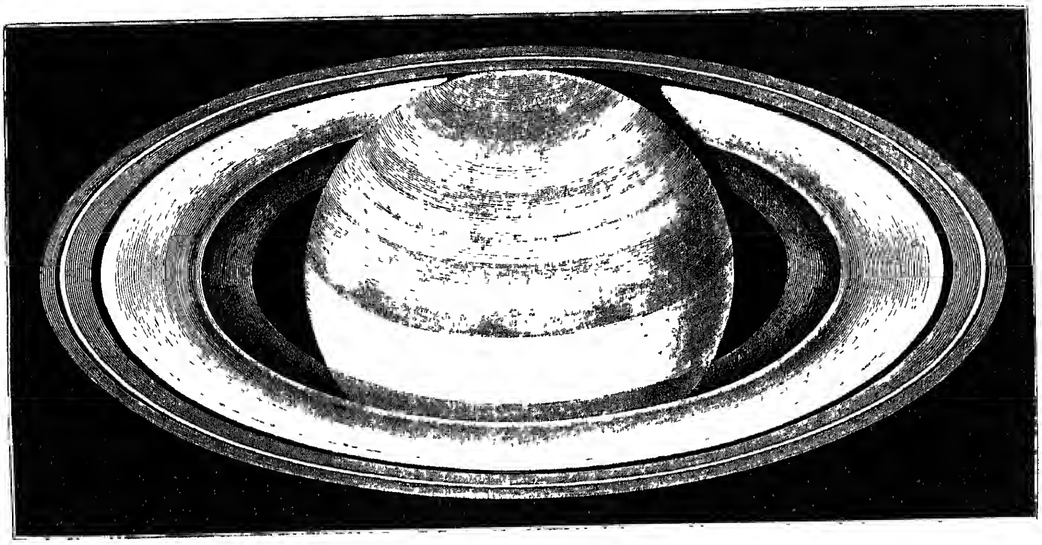
We are, however, ahead of our story! Without being aware of the American observations, on the 25th of that same month of November, Mr.

* I had the pleasure of proposing this able geometer in 1852 for a foreign membership of the Royal Society; and he was the first United-Statesman thus honoured; for Franklin was born a British subject, and was a regular F.R.S., having been elected into that body in April 1756.

Dawes was astonished on finding this same unexampled phenomenon. He immediately made a diagram of it in his journal, which drawing he soon afterwards brought to my house in London to inform me of the curious incident; wherefore I feel perfectly intimate with its independent English discovery. On the 3rd of December he shewed it to his friend Mr. Lassell, then on a visit to him at Wateringbury, and who was thereupon convinced of the existence of a new and dusky ring next to the body of Saturn. The further interesting researches of Mr. Dawes on this alluring topic, on the years 1851 and 1852, are detailed in the publications of the Royal Astronomical Society. This truly-zealous observer suggests, as worthy of consideration, whether the fact of the recent emergence of the southern side of the ring, after a total absence of sunshine for fifteen years from it, may not have had some effect in rendering the novel feature visible: and he conjectures, with Professor Pierce and Mr. Bond, that the semi-transparency of the marvellous aureole is due to its being composed of water, or some similar fluid. Time and ulterior means will probably yield satisfactory explanations; but meanwhile it is clear that we have an additional Saturnian hoop revealed to us, apparently consisting of matter which reflects light much more imperfectly than the planet, or the older known rings; and, what is still more extraordinary, transparent to such a degree, that the body of the ball can be seen through it. So obvious an evidence of physical action occupied a considerable share of my attention, but with little other result than exciting my wonderment. With the Hartwell equatorial the stranger has a very crape-like aspect, and, from its inferiority in brightness to the other rings, it resembles a bevelled or chamfered continuation of the series, for I could not define the black interval revealed by the gigantic refractors at Washington and Pulkowa. For the fullest details and measurements of the ball and rings of this most puzzling system—this living evidence of cosmogonic power—I must recommend the Uranian reader to the Annals of the Astronomical Observatory of Harvard College (vol. ii. part i.)

The following is an accurate representation of the Saturnian disc and hoops, as seen by Mr. Warren de la Rue on the 27th and 29th of March, 1856,

with his Newtonian reflector of 13 inches aperture. He obligingly presented me with the original beautiful drawing by himself, from which this exact xylographic engraving was cut by Mr. Thomas Cobb:—



But hold! Is the unique phenomenon herewith shewn, really and altogether a new discovery, or a restoration? For it seems, that, so far back as the Newtonian days, Hadley had remarked "that the dusky line which, in 1720, he observed to accompany the inner edge of the ring across the disc, continues close to the same, though the breadth of the ellipse is considerably increased since that time. On the other hand, neither Herschel 1. nor Schrœter produce any evidence of the dusky appendage having been discerned, although both those acute observers made remarks on the blackness of the intervening space; nor does a suspicion of its existence seem to have struck the elder Struve, when he made his admirable measures of this planet in 1826.* How such men, armed with such powerful instruments, watched Saturn closely

* Yet Schiller has been impressed into the service, being supposed, among the objects that he describes, to allude to it as that which lies before all eyes, surrounds it for ever, but is seen of none:—

"Das allen vor Augen
Liegt, euch ewig umgibt, aber von Keinem gesehn."

without suspecting the dark ring's existence, is really a matter of marvel, unless it is allowed to intimate a periodical visibility.

At length a shade of revealment on this point came to hand, for my worthy correspondent, Padre Secchi, was assured that the penumbra inside the ring, as well as its projection across the ball, had been remarked at Rome ever since the year 1828; yet without any further attempt being then made to ascertain its nature. In June 1838, the dusky appearance was distinctly perceived and duly reported by Dr. Galle at Berlin. Still this attracted little attention: indeed, though actually seen, its composition was not comprehended, nor was the wonderful appendage even recognized as a ring. But in the following summer he again caught sight of it, measured its breadth as 2·125″, and made drawings thereof. He now described it as a gradual shading off of the inner ring towards the surface of the planet, as if the solid matter of the ring were continued beyond the limits of its illuminated part—this extension of the surface being rendered visible by the emission of a very feeble light, such as would attend a penumbra upon it. Also in 1839, M. Mädler remarked on the constancy of the equatorial belt; and that *the dark zone attached to the ring* was due to the attraction of the ring upon the waters and atmosphere of the planet, giving both observation and theory.

In rê the Satellites of Saturn. Shortly after my account of those bodies was published in the Cycle, (vol. I. pages 197—202), I received a letter from Sir John Herschel, dated from Collingwood, November 29th, 1845, from which the following extract, pointing out a curious relation between the periods of the four interior satellites—was communicated by me to the Royal Astronomical Society's Meeting, on the 12th of the following December:—

Being on the subject of the satellites of Saturn, I will mention here a singularity which, though obvious enough, has not (so far as I am aware) been noticed before, viz. that the periodic time of the first satellite (first in order from the ring) is *precisely* half that of the third, and the periodic time of the second *precisely* half that of the fourth. This is far too remarkable and close a coincidence to be merely casual, and (the second satellite being a certainty) the extension of the law to the first (a law so out of the way and unlikely) would of itself be evidence of its real existence, even had it not been (as it now certainly has been) re-observed. If such atoms perturb one another's motions, there must be some very odd secular equations arising from this singularity.

It is not worth while to make a formal communication of such a thing to the Astronomical Society; but if you think it worth your verbal mention at the meeting, it may be interesting to those (if any) who are busy about satellitary perturbations."

In Vol. I. of the Cycle, page 201, I remarked that confusion had arisen from the indefinite nomenclature of these satellites, whether according to the their order of discovery, or their order of distance; for these two methods could not be brought to coincide. Since that time Sir John Herschel has proposed some well-devised mythological designations for them, beginning with that nearest to the planet, and finishing with the exterior one; and their adoption, though somewhat of a tax upon memory, will necessarily correct the disorder. But the proposition must be given in his own words:—

In the nomenclature above used the satellites reckon outwards from the centre, seven in number; but as this practice is even yet not become general among astronomers, and as I have found the equivoque practically annoying, and a source of frequent error and mistake, I have used, for my own convenience, and shall continue to do so in what follows, a mythological nomenclature, which however I do not presume to recommend to the adoption of others, though I am persuaded that *some* nomenclature other than the equivocal one now in actual use will be found necessary by all who observe these bodies. The names I have followed are as follows:—for

The exterior satellite, discovered by Cassini	Japetus.
The bright satellite discovered by Huyghens	Titan.
The exterior of the three satellites discovered by Cassini . .	Rhea.
The intermediate of these three discovered by Cassini . .	Dione.
The interior of them discovered by Cassini	Tethys.
The exterior of the two discovered by Sir W. Herschel . .	Enceladus.
The interior, and smallest of all	Mimas.

As Saturn devoured his children, his family could not be assembled round him, so that the choice lay among his brothers and sisters, the Titans and Titanesses (*vide* Lempriere.) The name Japetus seemed indicated by the obscurity and remoteness of the exterior satellite, Titan by the superior size of the Huyghenian, while the three female appellatives class together the three intermediate Cassinian satellites. The minute interior ones seemed appropriately characterised by a return to male appellatives, chosen from a younger and inferior, though still superhuman, brood. Should an *eighth satellite* exist, the confusion of the old nomenclature will become quite intolerable. I am not aware that a distant satellite of Jupiter (analogous to Japetus and our moon) has ever been *looked* for,—would it not be worth a search?

The seventh satellite, Mimas, has been deemed the *experimentum crucis* of performance in the largest telescopes, and few were the eyes that had perceived it even with them; and I must own that all my endeavours to make a personal acquaintance have been repulsed, with perhaps the exception of the instance stated in the Cycle, page 202. Here astronomers had rested for sixty years, when the eighth satellite, which had indeed loomed to the almost prophetic mind of Herschel, started into view: for though Saturn was already the most richly furnished of any of the known planetary systems, there was this other moon detected on the 19th of September, 1848. Now what rendered such discovery still more impressive, is the singular fact that Mr. William Cranch Bond in America, and Mr. Lassell in England,* both became aware of its existence and character nearly at the same time. Lassell's report, with his suggestion to name the stranger Hyperion, was placed before the Royal Astronomical Society previous to any information having been received from Professor Bond (see the Monthly Notices, viii. p. 195). On the 10th of November of the same year, I happened to be in the chair of that society when Mr. Bond's welcome announcement was made; and I communicated orally some further details on the subject which I had received from my intelligent and valuable correspondent, the Hon. E. Everett, as evidenced by the following extract, forming a striking incident in the history of the advent:—

> What a singular coincidence that a discovery so nearly simultaneous should have been made of the new satellite of Saturn at Cambridge (in America) and at Liverpool! In a short paper that I read to the American Academy, announcing the discovery (before we had heard from Mr. Lassell) I proposed to call it Prometheus, Hesperus, or Hyperion, all names from the family of Saturn. Mr. Lassell, in a letter to Mr. Bond, written before the news of the discovery at Cambridge had reached Liverpool, proposed Hyperion. The American Academy referred the subject of a name to a committee, and the committee left it to Mr. Bond, who, as the first discoverer, seemed best entitled to give the name. He decided for Hyperion.

* Of course this interesting discovery being made in Mr. Lassell's observatory, with a telescope moreover of his own making, he is entitled to a full recognition; but it ought to be added that his friend Mr. Dawes was in the observatory with him at the time, and suggested the method by which the stranger was proved to be a satellite in the short space of two hours and a half. This was by calculating the hourly motion of Saturn, and applying the micrometer, instead of mapping and diagramming the positions of the objects for comparison on the next fine night.

The elements of this new body have not been accurately ascertained as yet; though we accept, as being near the mark, that its distance from the parent-planet is about 940,000 miles, and its period for passing around that primary may be estimated at 21 days 4 hours and 20 minutes, in an orbit more eccentric than is the case of any other satellite.*

URANUS (Cycle, I. page 202). I stated that this planet was, *of all those yet known*, the most remote from the sun, so that its orbit consequently embraces those of all the others. This supremacy, of which it had so recently deprived the long invested and mighty Saturn, endured but for a short time; for since I made that remark, the wondrous introduction of Neptune has for ever placed a stopper on the usurper, increasing our boundary to an extent which would have surprised those who *demonstrated* that Saturn's rings, satellites, and size, palpably equipped him for the outpost sentinel of our spheres.

After what was advanced under β^2 Capricorni (Cycle, I. page 476), and having been assured by the best possible authority that "they are the devil's own objects to observe," I no longer sought a familiarity with the satellites of Uranus; but, the planet becoming importantly interesting from the theoretical researches of Le Verrier and Adams, and its position since then having annually improved in location for observers, I several times took advantage of a lucid and diaphanous sky to scrutinize his body. My hope was, by raising the planet's spurious disc as high as it would bear a sharp margin, to ascertain whether I could detect any oblateness, or ellipticity, in its form; but under every management it always appeared circular. Larger instruments have been applied to the same object, without gaining any direct evidence of an appreciable difference in the polar and equatoreal diameters,

* On quitting Hyperion, I ought to add another extract from Mr. Everett's letter, for its ingenuous warmth: "I wish you would procure for Mr. Bond senior (William Cranch Bond), the honour of an election to the Royal Astronomical Society. He is an observing astronomer of the highest order, accurate, untiring, possessed of marvellous optical quickness and tact, and modest to a fault." I am happy to say that Mr. Bond was unanimously elected an Associate of the Society on the following 12th of January, 1849. He died on the 20th of January 1859. The vacancy thus made was filled by the election of his distinguished son, Mr. George P. Bond.

so that both Otto Struve and Lassell pronounce Uranus to be round. This, however, from obvious causes, is as yet problematical; though, from the improved means in the hands of observers, there are strong reasons to hope that all doubts will be cleared off ere long.

SATELLITES OF URANUS. Perseverance and measures of the highest order have been devoted to these minute yet very suggestive bodies, since my account was printed (Cycle, I. pages 207-210). In that brief summary I tabulated the periodic times of the second and fourth satellites, as deduced from the observations of the two Herschels and Lamont; but a new master-mind subsequently discussed the affair, and the numbers now stand thus: –

SECOND SATELLITE.

	D.	H.	M.	S.
Period, according to Sir W. Herschel's later observations .	8	16	56	5·2
Sir John Herschel, by a comparison of his own observations with his father's	8	16	56	31·3
Dr. Lamont, from measures taken at the Munich Observatory .	8	16	56	28·55
Mr. Adams from the combination of all the observations between 1787 and 1848	8	16	56	24·88

FOURTH SATELLITE.

	D.	H.	M.	S.
Period, according to Sir W. Herschel's latest calculation .	13	11	8	59·0
Sir John Herschel, by a comparison of observations between 1787 and 1832	13	11	7	12·6
Dr. Lamont, from Munich observations . . .	13	11	7	5·92
Mr. Adams, from the whole series of observations . .	13	11	6	55·21

A considerable degree of uncertainty still obtains, regarding those satellites which Sir William Herschel discovered at a later period than the above two. The more recent observations by Mr. Lassell at Liverpool, and Otto Struve at Pulkowa, have decided the existence of at least two moons within the orbit of Herschel's second, or the closest of the brighter ones. Struve seems to have detected a third satellite, perhaps the interior one of the elder Herschel; and Lassell also observed another, which was presumed to be the one brought

forward by Struve; but this, agreeably to the computations of the Rev. W. R. Dawes, could not be the case, since Lassell's object would seem to have a periodic time of $2^d\ 2^h\ 39^m\ 36^s$, while the observations of Struve can only be satisfied by a period of $3^d\ 22^h\ 8^m\ 35^s$. This was about all we knew with respect to these matters in the beginning of 1848; and on the 11th of January, 1853, Mr. Lassell wrote from Valetta, in Malta—where he had established his 20-foot equatoreal telescope—as follows:—

> Surveyed Uranus with (*power*) 1018 under a fine sky and admirable definition, the disc appearing perfectly round, and having a remarkably hard and sharp edge. On this occasion, as well as many others, which I have not thought it necessary to particularise, I carefully scrutinized the neighbourhood of the planet to the distance of 5′ from his centre, for the discovery of other satellites. In the course of this scrutiny I made many measurements and diagrams of the positions of small points of light, which all turned out to be stars; and I cannot now resist the conviction, amounting indeed, in my own mind to certainty, that Uranus has no other satellites visible with my eye and optical means. In other words, *I am fully persuaded that either he has no other satellites than these four; or, if he has, they remain yet to be discovered.*

Notwithstanding this declaration, the question is far from being settled, and the result of the discussion seems to be that, at present at least, the periods and distances of four satellites are known with tolerable accuracy; and that, though several others undoubtedly exist, yet we require more definite information about them. Hence while some, in full reliance on the elder Herschel, and knowing the intricacy of the discordances, reckon eight of these followers to Uranus, astronomers in general restrict themselves to four only, which are by some considered to be mythologically designated after members of his family. But surely the names claim a more fanciful pedigree in fairy-land; nor is it possible to confound Shakespeare's elegantly beautiful Titania with the coarse Titana, nurse of the *Οὐρανίδαι*:—a wench who coolly countenanced bloody sacrifices and all sorts of child-murder, could never have danced, and sung, and sentimentalized as the gay Queen of the Fairies was wont to do.

The following are the approximate elements of the several satellitary orbits, the distances being expressed in equatoreal radii of their primary; and the periods of their revolution are given in mean solar days:—

No.	Name.	Sidereal Revolution.				Mean distance.
		d	h	m	s	
I	Ariel.	2	12	29	20 66	7·40
II.	Umbriel.	4	3	28	8 00	10.31
III.	Titania.	8	16	56	31·30	16·92
IV.	Oberon.	13	11	7	12·60	22·56

NEPTUNE. Since the publication of my Cycle of Celestial Objects, human intelligence has made one of the most magnificent discoveries that ever rewarded astronomical study, by adding an important link to our system, containing an amount of matter sufficient to form more than one hundred such worlds as ours. It is indeed a truly glorious developement: and when the grand result of bold analytical theorising from mysterious effects had led Reason to the exact position of an unknown attracting body, and to trace out its elements, the most enthusiastic admiration of the two great geometers, Le Verrier and Adams, was fully awakened throughout the civilised world. It was a triumph of intellect to which nothing in modern times can be compared, and it stamps an era which must for ever be memorable in the history of physical investigation: an exploit equally signal and edifying.

From causes pretty obvious, this advent was followed by feelings which in some quarters descended to misrepresentations, and perversion of facts; and, as circumstances placed me in contact with some of the consequent proceedings, I was much appealed to. Wishing therefore to give a general view of this deeply interesting incident to those officers who might have possessed themselves of the Cycle, I sent a somewhat detailed account to our professional periodical, the United Service Journal, in May, 1847. This was written off at the moment with all the impressions fresh, and was soon afterwards slightly enlarged and amended for private distribution. As a trusty friend thinks that the said *brochure* ought now to appear in the present volume, because copies of it are adrift, I have consented to reprint it in the Appendix. This leaves

me at liberty here to begin at once with the elements of the newly-recognized Neptune, as computed by Professor Kowalski—with several recent corrections—for the epoch 1850, January 1·0 Berlin mean time:—

Mean sidereal revolution (164·595 years) solar days	60119·40
Mean synodical revolution, solar days	367·49
Mean longitude	334° 36′ 29·0″
Mean orbital motion in a solar day	21·55707″
Ditto per annum	2° 11′ 13·86″
Longitude of perihelion	50° 16′ 39·1″
Inclination of orbit to plane of ecliptic	1° 47′ 0·9″
Longitude of ascending node	130° 7′ 45·3″
Eccentricity of orbit, half major axis as unity	0·0091740
Greatest equation of centre	1° 3′ 5″
Mean apparent diameter (37,646 miles) about	2·713″
True diameter, earth as unity	4·758
Mass, sun as unity ($\frac{1}{17133}$)	0·00005836
Mass, earth as unity	20·72
Volume, earth as unity	107·7
Density, sun as unity	0·763555
Density earth as unity	0·1924
Mean distance (2,862,175,000 miles) that of earth as unity	30·033861
Hourly motion in orbit, in miles	12464
Proportion of light and heat from sun, earth as unity	0·0011086
Mean arc of retrogradation	2° 50′
Duration of retrograde motion, in days	159
Apparent diameter of sun as seen from Neptune	63·93″
Greatest elongation of Uranus	39·8°
Greatest elongation of Saturn	18·5
Greatest elongation of Jupiter	10·0
Greatest elongation of Mars	2·9
Apparent diameter of Uranus in inferior conjunction	7·07″
Apparent diameter of Saturn in inferior conjunction	7·91
Apparent diameter of Jupiter in inferior conjunction	7·95
Apparent diameter of Mars in inferior conjunction	0·55

Such are the elliptical elements of a body which, at one swoop, extended our boundary from 1800 millions to nearly 3000 millions of miles from the sun; and, although this body obeys the mandates of solar attraction, yet, so vast is the distance, that it could not be drawn to the central body by the centripetal force in less than 13,532 days, or about 37 years, though falling

with a weight more than twenty times that of the earth. The terrestrial pound, from the greater size of the sphere, would weigh only 14oz. 10dr. on the surface of Neptune, and for the same reason a heavy body would descend through 14·73 feet only in the first second of time. Relative to the brightness of his day, and the amount of solar heat which he receives, it is plain enough by rule that, being 30 times more distant from the sun than the earth is, he receives only a 900th portion of the light and heat which we enjoy; but this estimate rests on the supposition that his atmosphere resembles ours, that the solar light is received under like physical conditions, and that it acts upon visual organs of a similar structure with ours; which assumptions are by no means unquestionable. Were a well-adjusted railroad established between the sun and Neptune, an express train averaging 35 miles per hour, without stoppages night or day, would be 9,329 years on the journey between those celestial orbs: but that subtile agent, light, which we know moves with a velocity that could carry it eight times round our globe during one beat of a pendulum, would be able to perform the feat of arriving at our present outpost in 4h. 9m. 12s.

Here then is Neptune, our hitherto unknown companion, rolling on in ceaseless majesty, and describing a grand orbit, which beyond doubt is stable, notwithstanding some apparent anomalies. It may have revolutionary inequalities of very long duration from reciprocal attraction; and Mr. Adams, with untiring energy, has already computed that Neptune produces a periodical perturbation in the motion of Uranus, the circuit of which is 6,800 years.

A satellite of Neptune was discovered by Mr. Lassell, at the Starfield Observatory, in October 1846, and the following are its details:—

Sidereal revolution around its primary	$5^d\ 21^h\ 3^m$.
Apparent semi-axis major of orbit	16·98″.
Actual mean distance from Neptune in miles . .	235,800.
Inclination of orbit to ecliptic about . . .	29°.
Apparent magnitude as seen from the earth . .	14th.

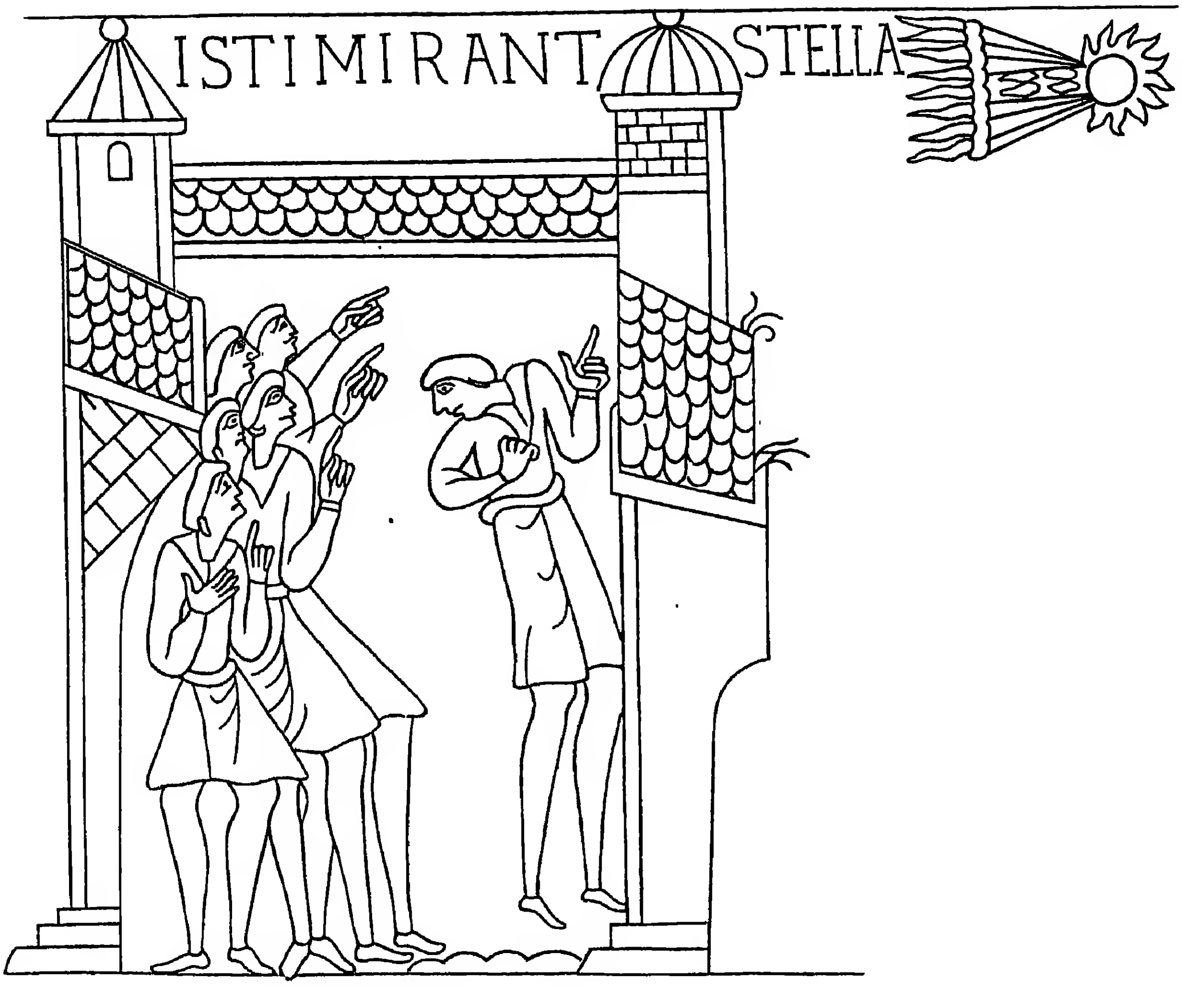
The appearance of this planet is that of a star of the 7th or 8th magni-

tude, and in the Hartwell telescope it is a well-defined circular body, with a steady pale greyish tint; but I certainly did not fully appreciate the grandeur of this *Ultima Thule* until I saw both him and his satellite in Mr. Lassell's gigantic equatoreal at Liverpool, on a beautiful night in September 1856; I was then indeed awed, for the object was grave and suggestive!

Here, by the vouchsafed powers of mind and hand, a world unthought of from the Creation down to our day was before me—yet one which had traced its orbit for an infinity of ages, as a component body of our system. The sight recalled Young's vision of orb above orb and circle in circle without end—

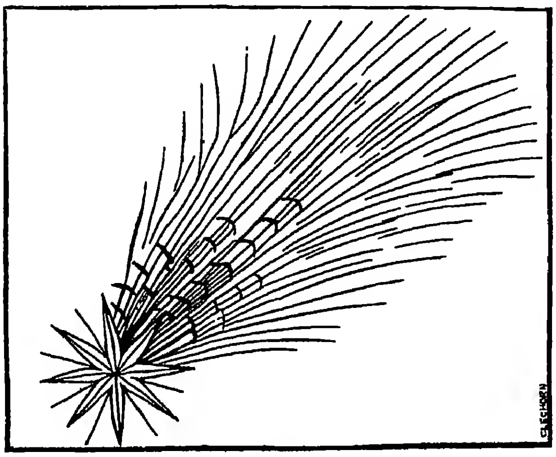
> What involution! what extent! what swarms
> Of worlds, that laugh at *Earth!* immensely great!
> Immensely distant from each other's spheres!
> What, then, the wondrous *space* through which they roll?
> At once it quite ingulfs all human thought;
> 'Tis comprehension's absolute defeat.

HALLEY'S COMET (Cycle, I. page 213). I made mention of a Norman Chronicle which showed the right divine of William to invade England—"how a star with three long tails appeared in the sky; how the learned declared that such stars appeared only when a kingdom wanted a new king; and how the said star was called a *Comette*." Since that was published I have carefully tried back—upon intervals varying from 76 to 79 years as the duration of each revolution—to see what ancient notices of comets would fit the perihelion passages of Halley; and the result strongly indicates that the blazing star which alarmed Europe in 1066 was the one in question. As it arrived upon an important occasion, so it assumed an aspect which excited astonishment and dread, being equal to the full moon in magnitude, with a train, or bunch of tails, at first short, but which increased to a wonderful length. The writers of the eleventh century bear full testimony to its splendour: and according to the evidence of the Bayeux Tapestry, embroidered by the English damsels of Queen Matilda, the Conqueror's exemplary wife, it thus fixed the gaze of the doomed King Harold's courtiers—



This grotesque representation, executed in coarse needle-work, may be considered a tolerably fair though very ugly likeness of Halley, at that apparition: and it is probably its earliest authentic portrait. The old Nuremberg Chronicle indeed exhibits (fol. clvii.) it for the year A.D. 684, but from what authority the staid xylographers of that recondite work took it, does not appear. Mr. Hind, in his descriptive treatise on Comets, gives the epoch of its perihelion passage as 684·80; and he says that, in the same year, "Ma-tu-an-lin, the Chinese historian, makes mention of a comet which was observed in the western heavens, in September and October. This agrees with the course of Halley's, because its perihelion passage fell about the middle

of the latter month, and, as the interval would agree well, we may infer that there is a fair probability of their identity." According to the Nuremberg sages, this advent portended as great misery as three successive months of heavy rain, thunder, and lightning could bring—such a destructive elemental scourge, indeed, as was never before known in Rome or Italy. The following is a fac-simile of the effigies there given of it:—



Stella crinita Anno xp'i 684.

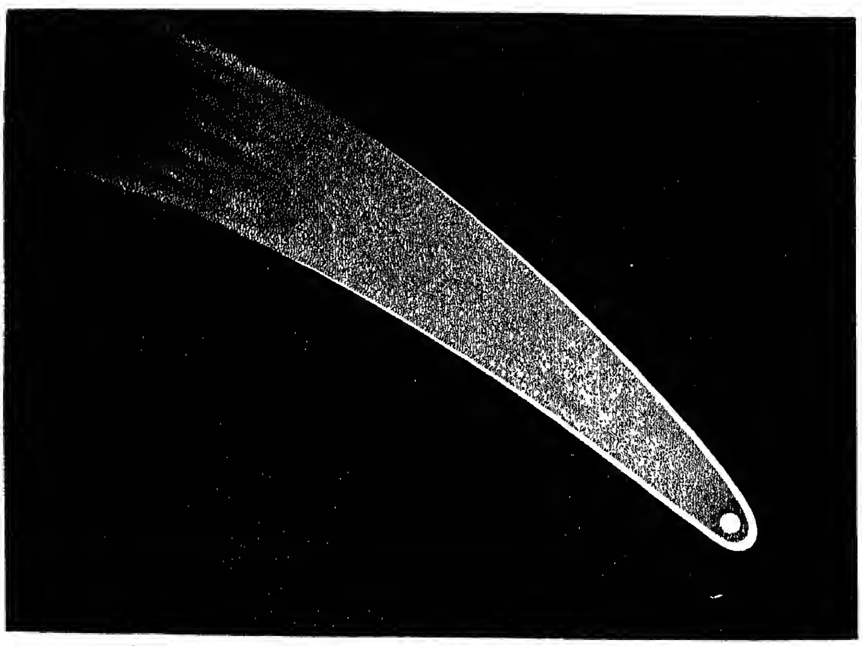
Comet of 1680 (Cycle, I. pages 224 and 227). The periodic time of only 575 years, assigned by Halley to this body, requires revision, since Encke and other computers with increased experience, deriving that element from the excentricity and the perihelion distance, are inclined to extend it to 805, if not to 8800, years. Halley's short period therefore would allow of an aphelion distance only one-fifth of that deduced by Encke.

Comet of 1811 (Cycle, I. pages 218-219). This grand body, with its enormously bulky head, has been much discussed since the apparition of Donati's beautiful stranger of 1858, as to the respective splendour of the two. Indeed I was so frequently appealed to about them, that I wrote to Mr. Grant, the Editor of the Royal Astronomical Society's Monthly Notices, and will here extract the passage from the xixth volume of that work (pages 27 and 28):—

In *re* the magnificent comet,—I have been closely attending to its fine figure, and am asked on various sides, as I had the advantage of having minutely watched both, which I thought the

most splendid in appearance, this or that of 1811? Now, to my memory, which is very distinct, the palm must be given to the latter. As a mere *sight* object, the branched tail was of greater interest, the nucleus with its "head veil" was more distinct, and its circumpolarity was a fortunate incident for gazers. But, recollect, that in these remarks I mean nothing disrespectful to the *Donati*. On the contrary, with those exceptions, it is one of the most beautiful objects I have ever seen in the heavens. The head is certainly not so fully pronounced as in that of 1811, but greatly is its physical interest increased by segments of light and a dark hollow, giving the aspect a resemblance to the gaslight called a bat's wing. This dark line or space down the centre of the brilliant phenomenon, not only had the direct tendency to stregthen the luminosity of the jets of light, in the manner observable in the burning of a wax taper, but also, on a fuller scrutiny of this singular characteristic, to recall its striking resemblance to the similar feature seen in water-spouts, and in the pillars raised in sand-storms which I have witnessed in North Africa.

But, in order to show this matter more clearly, I have selected from a publication of the day, the following view of Flaugergue's comet as it appeared during the autumnal months of 1811, with a bifurcated and lengthy train, a view which really recalls it well to my recollection:—



COMET OF 1811.

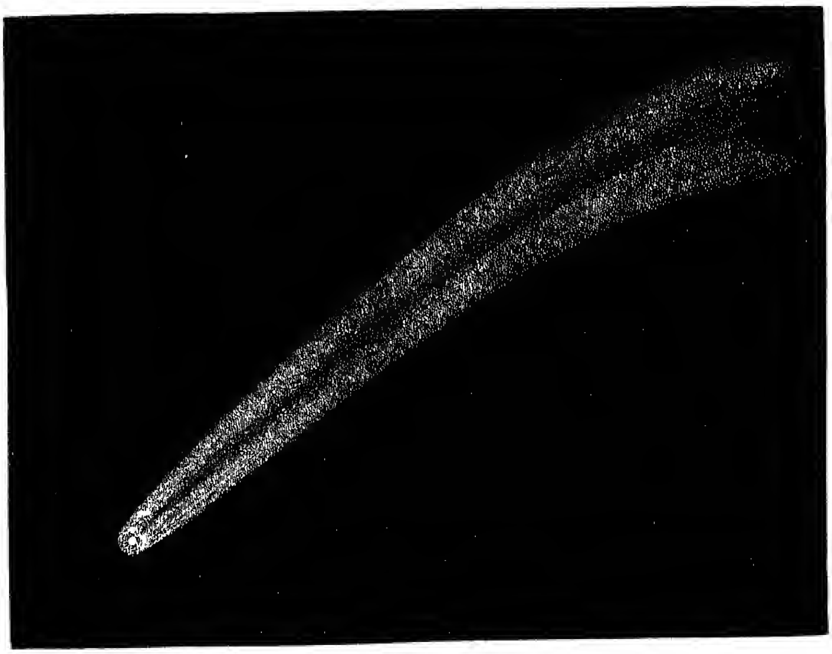
Yet this wandering body is calculated to recede from the sun to a distance fourteen times that of Neptune. THINK!

Mr. Maclear, the indefatigable Cape astronomer, writes that he remembers

the comet of 1811, and considers that neither it, nor Donati's, are comparable in splendour to the great one which appeared in 1843 (see Cycle, I. page 247). This assertion is also borne out by two paintings in my possession—night and day views of that glorious phenomenon—by my son Charles Piazzi Smyth. On these grounds then, added to its descriptions and measurements, with a tail of more than 60° in length, the great comet of 1843 must be rated the finest and most remarkable, as yet, of the present century, though my own experience, from my being in English latitudes, was limited to perceiving with an opera-glass, a long and narrow, pale, luminous streak in the S.W. regions, between Orion and Lepus. It indicated, however, a tremendous comet; and even imagination is overpowered on recollecting that this wanderer, under the reins of gravitation, carried its perihelion approach to such unexampled proximity as to drag its planetary nucleus to within 95,000 miles of the solar surface, which, says Herschel, exposed it to the heat of 47,000 suns such as we experience the genial warmth of! The physics of these bodies are under strict and increasing *surveillance:* insomuch that more will no doubt be known by astronomers respecting this admirable comet, about four centuries hence, when he shall have completed the period of his revolution.

The mediæval writers generally describe comets as glaring with fearful and terrible aspects, significant of malignance: but had they seen Donati last September and October, with its pure effulgence and graceful ostrich-feather-like inflection of the tail, they must have pronounced a different sentence. Those of the present generation who gazed upon its beauties, which includes all but those who regard stars as sheep do, are not likely to forget the magnificent spectacle exhibited to the naked eye between seven and eight o'clock on the evening of the 5th of October, when it glided majestically over Arcturus, without dimming its brilliancy in the least, although its head was only about one-third of a degree from the star. The latter shone remarkably white during the appulse, especially at 7^h 15^m, when it entered the central dark void of the cometary appendage—which was then flitting with unusual animation. The curvature of the tail, which might be estimated at about 24° in length,

appeared the more elegant from this incident, but it had indeed been equally so for several mornings and evenings to the unassisted eye, marked moreover with a peculiar brightness of feature on the convex side: and the application of a binocular opera-glass, magnifying only three times, certainly represented the characteristics on the 30th of September, as engraved below:—



DONATI'S COMET.

At this time it was still about 70,000,000 of miles from us, and therefore its physical aspects were somewhat puzzling: but the astronomers on the planet Venus must have had a glorious view of this comet, since it passed them within a ninth part of the Earth's mean distance from the sun, wherefore they had a narrow escape. Here the oscillatory motion of the nucleus—the hooded sectors and streams of light in rapid evolution—the cuspidated disks and aureoles – and the dark axial space down the envelope which so greatly perplexed our vision here, could be well examined there, unless terror scared observation away.

COMET OF LONG PERIOD (Cycle, I. page 248). Since I mentioned that the great comet of 1264 and 1556 should be looked for again, it has received

much attention from the cometary investigators; among whom Messrs. Hind and Bomme have particularly distinguished themselves. Some wild predictions gave as much alarm to the excitable Parisians, as Biela's did in 1832 (Cycle, I. page 254), and the terrors of Charles V. seemed to be revived. Yet the alterations in the elements required to bring about a collision, are too large to be admissible, notwithstanding there is some degree of dubiousness in the determination of the orbit in 1556, deduced from observation only. The chance of our being enveloped in the tail, or having it sweep over us, is of course greater. The time of its re-appearance, supposing the comets of 1556 and 1264 to be identical, is doubtful between 1857 and 1861, owing to the impossibility of fixing the position of the perihelion and inclination of the orbit in 1556—from uncertainty in the data—within sufficiently narrow limits to allow of an exact computation of the planetary disturbances.

Comets of short period (Cycle, I. pages 249—256). Since the publication of my former work, this branch of astronomy has been cultivated with such attention, that instead of the three—Encke, Faye, and Biela, which I therein recorded—the short-period comets are now increased to six, with a fair promise of more; and they stand thus in order of discovery:—

Names.	Aphelion dist.	Perihelion dist.	Period.
	Miles.	Miles.	Years.
Encke . .	390.010,000 . .	32,120,000 . .	3·296
Biela . .	590,100,000 . .	81,600,000 . .	6·617
Faye . .	565,200,000 . .	161,300,000 . .	7·440
De Vico .	478,300,000 . .	113,300,000 . .	5·469
Brorsen . .	537,750,000 . .	61,950,000 . .	5·581
D'Arrest . .	547,900,000 . .	111,900,000 . .	6·441

There are six others that were suspected of describing small elliptical orbits at the time they were discovered, but whose periods are not yet accurately ascertained: they are, one seen in 1743, Messier's of 1766, Pigott's of 1783, Pon's of 1819, Blanpain's of 1819, and that of Peters in 1846. These are mostly nebulous patches, which, but for their highly interesting physics,

would be barely worth looking at. Of these, Encke may prove the most valuable (Cycle, I. pages 253—5; and Chapter IX. of this book), as being the first and probably the best for establishing the painful and humiliating fact of the existence of a resisting medium—an hypothesis involving the final destruction of our whole system in the course of ages: but Biela* has kindled the greatest alarm (Cycle, I. pages 253—5), from its orbit nearly intersecting ours, especially at the nodal passage of 1832. I adverted to the prevalent consternation of that day, although perhaps there would have been but little danger to us in case of a collision, for it seems pretty certain that the earth would pass through the gaseous intruder like a cannon-ball through a fog-bank. Since my description, however, of this all but invisible object, it exhibited a most astounding—because as yet unexplained—phenomenon during its apparition in 1846. On the night of the 13th January of that year, as Lieutenant Maury, of the Washington Observatory, was gazing at it, he perceived another small comet, one or two minutes to the north, and about the same distance to the west, nothing of which was visible on the 12th. This strange and unprecedented appearance was also detected in England by Professor Challis on the 15th of January: he informed me—as President of the Royal Astronomical Society—of the fact in a letter from the Cambridge Observatory, dated January 23rd, 1846, at 9^h p.m.—

I hasten to communicate to you a most extraordinary fact about Biela's comet. On the evening of Jan. 15, when I first sat down to observe it, I said to my assistant, 'I see *two* comets.' However, on altering the focus of the eye-glass, and letting in a little illumination, the smaller of the two comets appeared to resolve itself into a minute star with some haze about it. I observed the comet that evening but a short time, being in a hurry to proceed to observations of the new planet. Casualties and press of occupation prevented my observing the comet again till this evening. On first catching sight of it, I again saw *two* comets. Clouds immediately after came over and continued to obscure the comet for half an hour. My suspicions were roused. I searched to see whether any nebula was near the comet's place, but could find none in the catalogues. When the

* No one valued poor Gambart—as well for his *bon-hommie* as for his talents—more than I did: but, though he independently discovered this comet at Marseilles on the 9th of March, 1826, Biela had caught it up at Josephstadt ten days sooner, namely on the 27th of February. Now as both astronomers computed its orbital elements, and both recognised its periodicity, Arago's illiberal complaint about the name must be allowed to evaporate.

clouds cleared off, on resuming my observations, I suspected at first sight that *both* comets had moved. This suspicion was abundantly confirmed by ulterior observations. The two comets have certainly moved in equal degree, retaining their relative positions. I compared both with Piazzi 0.120, and the motion of each in 50^m was about 7^s in Æ and 10″ in N.P.D. What can be the meaning of this? Are there two independent comets? Or is it a *binary* comet? Or does my glass tell a false story? I incline to the opinion that this is a binary or double comet, on account of my suspicion on Jan. 15. But I never heard of such a thing. Kepler supposed that a certain comet separated into two; and for this Pingré said of him, "Aliquando bonus dormitat Homerus" I am anxious to know whether other observers have seen the same thing: in the mean while I thought, with the evidence I have, I had better not delay giving you this information. I will write again immediately that I am able to confirm this puzzling phenomenon.

Being in Edinburgh when I received this communication – whither I had repaired to plant my son C. P. Smyth on Carlton Hill—I showed it to my valued old friend Sir Thomas Brisbane, President of the Royal Society of that metropolis; and, as we went there together in the evening, the letter was read to the meeting by Professor James Forbes, and excited great attention. Three days afterwards, Mr. Challis again addressed me on this subject:—

I beg to lay before you the following additional particulars respecting the present extraordinary appearance of Biéla's comet. There are certainly two comets. The north preceding is less bright, and of less apparent diameter than the other, and, as seen in the Northumberland telescope, has a minute stellar nucleus. I compared the two comets independently with A. S. C. 51 on the evening of Jan. 23 and Jan. 24, and obtained the following places:—

Preceding Comet.

Greenwich mean time.	Æ of Comet.	N. P. D. of Comet.
Jan. 23, 7^h 8^m $11{\cdot}9^s$	0^h 25^m $56{\cdot}68^s$	91° 9′ 30·1″
24, 7 1 19·2	0 29 18·40	91 14 35·2

Following Comet (Biéla's).

Greenwich mean time.	Æ of Comet.	N. P. D. of Comet.
Jan. 23, 7^h 1^m $17{\cdot}7^s$	0^h 26^m $0{\cdot}88^s$	91 11 31·6
24, 7 2 58·5	0 29 23·51	91 16 42·1

By reducing the places of the following comet to the same epochs as those of the preceding comet, I find:—

Greenwich mean time.	Diff. Æ of the Comets.	Diff. of N.P.D. of the Comets.
Jan. 23, 7^h 8^m 11·9	5·18	123·0″
24, 7 1 19·2	5·11	126·6

Hence

	Angle of Position.	Distance.
Jan. 23	327° 43′	145·4
24	328 48	148·0

The greater apparent distance between the comets on January 24, is partly accounted for by their approaching the Earth. The increment of apparent distance on this account in one day is 1·6″, supposing both comets to be nearly at the same distance from the Earth. I saw the comets again yesterday evening, but took no observations. The relative positions were apparently unchanged. I think it can scarcely be doubted from the above observations that the two comets are not only apparently but really near each other, and that they are physically connected. The smaller one when I saw it on January 15th was faint, and might easily have been overlooked. Now it is a very conspicuous object, and a telescope of very moderate power will readily exhibit the most singular celestial phenomenon that has occurred for many years—a double comet.

We were all, as it were, taken aback; from what had hitherto been examined as a single body, thus seemingly disintegrating into two distinct systems, each with a nucleus and tail of its own. There could exist no doubt as to the reality of the phenomenon, as their distance increased at the rate of about 1′ in arc per week. But the evidence is not sufficient to explain how Biéla divided into two parts, yet allied by some inscrutable bond—even to occasionally throwing slender gaseous arcs across to each other; and alternating reciprocally in brightness, they continued their journey together through space for upwards of three months, before they receded out of sight. From these duplex nebulosities appearing to be governed by some mysterious law, many have held the separation to be owing to a repulsive force having overcome the central attraction of the mass. Here the energy of the electric in comparison of the gravitating force, as exerted on matter of equal inertiæ, is brought under suspicion: this is treading on hot ashes,—but in August and September, 1852, they were seen again by several astronomers, after the completion of their usual orbit, still similarly situated relative to each other! Yet, after all the various explanations and "opinions quaint," which have gone forth, the occurrence may only be owing to two distinct comets, one occulting the other, and, as the nearest hove ahead of the outer one, a relative change of position and distance would take place: still the retrocession was not rapid enough to indicate much difference. There is now reason to think that, by being better prepared for each periodic return, and from the zeal and ability of an increasing corps of observers—armed with improved means of observing, and a scrupulously exact method of registry—we shall learn something more about them, and thus extend

our critical insight into the nature and condition of those ethereal bodies, as well as of the regions through which they roam.

A GLANCE AT THE SIDEREAL HEAVENS.

THE INERRATIC SPHERE (Cycle, I. page 267). Since that page appeared, the wonderful expanse between our solar system and the stellar regions has been probed with such skill and perseverance, that we now deem Parallax to have been finally detected in eight several outposts of the incomprehensible celestial boundary; if that can be termed boundary which has no limit but in the weakness of our faculties.

In 1847, the elder Struve printed his *Etudes d'Astronomie Stellaire* for distribution to his friends, among whom I had the honour of being included. In that interesting work, the able philosopher notices the speculations of Galileo, Kepler, Huyghens, Wright, Kant, Lambert, Michell, and Sir William Herschel, on the structure of the heavens and the distance of the stars, together with the applied labours of Argelander, Bessel, Mädler, Sir John Herschel, and his zealous son Otto Struve. He then proceeds to his own researches on the Milky Way, in which he determines the law of condensation of the stars towards a principal plane; and he calls attention to the singular view taken by Dr. Olbers respecting the extinction of sidereal light in its passage through celestial space. But the point to which we were chiefly drawn is the abstract of the then unpublished but truly grand inquiry of Mr. Peters, of the Pulkowa Observatory, on stellar parallaxes. On this authority the mean distance of stars of the first magnitude may be estimated at 986,000 times the radius of the Earth's orbit, or so remote, that their light only reaches us in 15½ years.

In pressing the peruser to pause a little on the mighty abyss which separates our known system from the stars, it may be useful to enter more into detail; and, although the human imagination can but faintly grapple with the astounding figures of the theme without using colossal units, we will have recourse to a scale of geographical miles, as more familiar to many readers than orbital radii. Quitting cannon-balls, then, and railway trains as "slow coaches," we must

recur to light, rushing along at the wonderful rate of eleven or twelve millions of miles in every minute of time.* Yet even this subtile element would require the number of years assigned below to traverse the several distances there enumerated, and which are considered to be pretty well established:—

Names.	Millions of miles distant.	Years.
α Centauri	21,508,000	3·6
61 Cygni	55,983,500	9·3
Wega	74,632,000	12·4
Sirius	130,435,000	21·7
Polaris	184,870,000	30·7
Capella	425,980,000	70·7
9th mag. stars (*Peters*)	3,696,000,000	586·0
31 Messier	260,870,000,000	43,312·0

A word upon this last instance, because, tremendous as its distance must be deemed, it is really but a stepping-stone to other objects existing within the reach of our assisted vision, but the light from which would require more than a million of years to reach us. 31 Messier, however, is in every way of deep importance to the sounding of space (Cycle, II. page 16), since Sir William Herschel, after a rigid scrutiny, concluded it to be the least distant of all the great nebulæ. By his elaborate gauges he considered its distance to be about 2,000 times that of Sirius, which was then held to be the nearest star to us, and not as inserted on the previous table. Cassini assigned Sirius a parallax of 6″, and Lacaille's determination of 4″ was confirmed by Piazzi (Cycle, II. page

* This assuredly is swift enough for our purpose; but Wheatstone shows that the velocity of electricity through a copper wire, exceeds that of light through space! The reason for our expressed contempt of that old exponent of swiftness the cannon-ball will be palpable when we state, that, while light might fly to us from Neptune in very little more than four hours, a 32-pound shot, driven by the old full-service charge of one third its weight in good powder, and going on at the initial velocity with which it quits the mouth of the gun, namely 1,500 feet per second, would require 320 years to perform the same journey.

Before quitting these long series of numbers, it may prove useful to amateurs to insert a warning, that billions among the continental nations are understood to contain three places of figures less than with us; that is, our billion is the compact epithet for 1,000,000,000,000, or a million of millions, whereas on the continent the epithet is changed to billions at a thousand millions, thus, 1,000,000,000 is one billion.

162). But, since the opinions of Herschel were promulgated, the masterly observations of Henderson and Maclear, made under all the latest instrumental refinements, have boomed off that beautiful star to a parallax of only 0·23″, so that, instead of the nebula's light requiring merely 6,000 years to arrive here as formerly advanced, it must take full 43,312.

The vast intermediate gulfs—insensible to any gravitation, which separate our system from the stars, the stars from each other, and the clusters still more from the nebulæ beyond them—are doubtlessly so placed by an Omniscient ordination, to maintain the stability of the whole. Yet no human mind can fully comprehend the magnificence, complexity, and harmony of so stupendous a scheme; for, if we were carried to 31 Messier, the sublime perspective beyond of orb upon orb, would still continue. Even thought itself fails in estimating the material universe: still men, and apparently sane men, have absurdly bestirred themselves to cast bonds around infinitude, materialize the design, and limit the faculty of the inscrutable CREATOR!

VARIABLE STARS (Cycle, I. pages 272-4). Since those pages were written, this very important class of celestial phenomena has been diligently watched from various stations, but more especially by Argelander abroad, and by Hind and Pogson in England; insomuch that the list of them has been very considerably enlarged and is still increasing. But it must be borne in mind, that variation in the light of certain stars is by no means a proof of periodicity, nor must he who gazes in full moonlight compare his observations with those caught during our satellite's absence, though the difference is simply the effect of an optical contrast. Yet, under the best available scheme of photometry, so general is the property of this stellar wavering of light, as to induce Herschel to think that about one in every thirty of the stars that are visible singly, exhibits an occasional measurable change of lustre; the fluctuations in some cases are very considerable, and the irregularities perplexing. It is perhaps one of the richest of the all but unexplored mines in extra-meridional astronomy, and those ardent *savans* who have taken the field, have had most encouraging success in breaking

ground; but, although our practical knowledge of periodically-variable luminaries is also greatly enlarging, their physical nature is still a sealed mystery.

Mr. Hind has noted that variable stars, especially the fainter ones, have generally a ruddy tint, on which Arago dilates thus: —"Might there not be some connexion between this remark and the observation made by the same astronomer, that variable stars at the instant of their minimum brightness appear surrounded with a kind of fog? Supposing the existence of this fog well established, we should be on the road to the explanation of these singular phenomena. Perhaps we may arrive at the conclusion that a star's variations of brightness are due, not to a perfectly opaque planet revolving round it, but to cosmical clouds, which, by a similar movement of revolution, would be successively interposed between those bodies and the earth." However this may prove to be, it must be received as a fact in the discussion, that nine of Hind's variables are recorded as being red, and one absolutely *crimson*, like to "a blood-drop on the black ground of the sky." The fog or haze at the minimum was palpably obvious, and, in several, very decided changes of colour took place at different stages of the period; for instance, R Geminorum, announced by its discoverer as passing through blue, yellow, and red during the gradations of its lustre, which is fully confirmed by Mr. Pogson. Besides the detection of the above-mentioned crimson star, Mr. Hind thus wrote to me from Regent's Park, under date of the 14th January, 1850:—

> I also avail myself of this opportunity to mention that, in October, 1845, I remarked a most fiery or scarlet star on the confines of Lepus and Orion Æ (1850) 4h. 52m. 45s. and NPD 105°. 2′. This star is *by far* the most deeply-coloured of any that I have yet seen, and in striking contrast with a beautifully white star preceding it one minute. It is not inserted in Sir John Herschel's Catalogue of Red Stars, there is no allusion to it in your "Cycle," nor can I find any previous notice of it; yet I am doubtful whether I can have first discovered this remarkable star.

Also on the night of the 28th of April, 1848, he suddenly perceived a red star of between the fourth and fifth magnitude, very conspicuous to the naked eye, in the region of the Serpent-bearer, in Æ 16h. 51m., and in Dec. — 12° 39′, where, from a thorough acquaintance with the spot, he felt assured, and could

state positively, that no star down to the tenth magnitude had previously existed; it was to unassisted vision as bright as ν Serpentis. I happened to be absent from home when Mr. Hind sent me notice of this "Mira Ophiuchi," nor was it till the end of June that I was able to get a telescopic glimpse of it, when it had dwindled to about the eighth degree of lustre only. From the time of its discovery the stranger continued to diminish, but without altering its position; and, before the advance of the season rendered observations impracticable, it had nearly disappeared. But, although it has been watched from 4·5 to below 13·5 magnitude, it cannot as yet be enrolled among the periodical variables, and for the present must remain under the designation of *Hind's Changing Star*. I may, however, just call attention to the Cycle (II. page 394), where mention is made of Kepler's scholars discovering a new first-rate star in the foot of Ophiuchus, which became invisible in a few months. Now, the "foot of Ophiuchus" is a loose term, especially in the drawings of that day, and the positions are sufficiently near to ask whether the new "Mira" may not be a re-appearance of the Keplerian evanescent star?

Some of these apparent irregularities may yet be found to be reducible to extremely regular cycles, for there is a vast deal of which we scarcely know anything, and much more that our knowledge of is of too short duration for us to penetrate the complicated system. Pigott's variable star in Corona is represented by Argelander to alter for the most part so little, that the unaided eye can hardly decide on its maxima and minima of light; yet, after the lapse of whole years of these slight and unsteady waverings, they suddenly become so great that the star completely vanishes. The variations of the red and brilliant Betelgeuze (α Orionis) were detected by Sir John Herschel in 1836, and were "most striking and unequivocal" until 1840; but, within the years since elapsed up to 1848, they became much less conspicuous. In 1849 the fluctuations again increased; and, on the 5th of December, 1852, Mr. Isaac Fletcher observed Betelgeuze to be brighter than Capella, and actually the largest star in the northern hemisphere! The indefatigable Argelander establishes the time of its increase at 91·5 days; he also states, that, from the twentieth to the seventieth

day after the maximum, its decrease is insensible; and Fletcher, having quoted my mention of Pollux in the Cycle (vol. II. p. 187) as being variously estimated by successive astronomers of the first, of the second, and even of the third magnitude, adds under date of the 20th of February, 1850, the following remark:

At the present moment this remarkable star, Pollux, is obviously and unmistakeably brighter than Castor; at any rate fully as bright as, if not brighter than, *α* Ursæ Majoris; though I cannot say how long it may have been so.

In proof of what extreme care should attend observations of every description, however unimportant they may be deemed at the time, I will now cite the remarkable case of R and S Cancri.* These two objects, preceders of what I have termed a coarse triangle, are so closely following a fine compressed cluster of very minute stars, 80 Messier, that with moderate magnifying powers they are in the field of view together (Cycle, II. page 357.) They were first diagrammed on the 30th of April, 1837, when the followers were jotted down as of the tenth magnitude ±; but, being then bent only on obtaining guide-posts to secure identity for amateurs, and not thinking of variables, I hastily marked them the same on the 7th of May 1839; at all events they were both visible at those times. Thus matters rested till the summer of 1853, when my obliging correspondent M. Chacornac found that R from the ninth magnitude had become invisible, or below the twelfth degree of lustre, while S remained of the ninth; but, on the 19th of May in the following year, S was invisible, and R of the ninth or tenth magnitude. "This coincidence," says M. Chacornac, "however rare, shows two variable stars side by side, and made me believe a displacement of S, a displacement that would have required an enormous proper motion in R. A series of micrometric observations soon dissuaded me from so exaggerated a supposition; and, on the 14th of April

* The capital letters of the latter division of the Roman alphabet have been applied by Argelander to the small variable stars which, before his time, had received neither name nor sign. Under his system he commenced with R, because others had used up from A to N; R therefore signifies the first discovered variable in any constellation, S the second, T the third, and so on. I regret to say that I think this nomenclature is inadequate to the object, for Gemini, Virgo, and Capricorn are running out fast to Z; and the rapid progress of recent discovery threatens the scheme with bankruptcy.

1855, I actually again saw S in its place of the ninth magnitude, that is, as brilliant as on the 6th of July, 1853." Five years after the latter date, I was informed that Mr. Baxendell could see neither of them, they having either disappeared or both dwindled down to less than the twelfth magnitude; whereupon I thought it best to hand them over to Mr. Pogson, together with facsimiles of my diagrams of 1837 and 1839. In an answer from this gentleman, dated Oxford, 2nd of July, 1858, he says, "I beg to return thanks for the configurations near R Scorpii, for which I felt greatly indebted, and forthwith examined the objects in question. Time only permits me to state in this brief note, that your epochs will greatly facilitate the determination of the periods of both R and S Scorpii. Chacornac's positions for 1852.5 are merely approximate:" and, in April 1859, he sent me word that "S Scorpii is now visible, but not R. Last year the contrary was the case about the same time, or a little later." I am satisfied that they are now in good hands.

The increased application that has been made to this interesting department enables me to supersede the list of Variable Stars given in the first volume of the Cycle, with a better. This I have tabulated in a style which would save it from the angry rebuke of M. Arago, who, in his Popular Astronomy (i. pages 429 and 250), exclaims—"In certain cases, in fact, the discovery of the periodic star has been attributed to the observer to whose eyes it first offered itself as a new star, although he never doubted that, after being enfeebled, it would return to its original brightness. In other cases, on the contrary, the astronomer who first recognised the periodicity has been entirely thrown overboard, and the whole honour of the discovery has been attributed to him whose observations, combined more or less skilfully with those of his predecessors, have assigned to the period the most accurate numerical amount." Yet Arago, instead of compiling a table himself on this principle, merely extracts a list of variable stars from Humboldt's Cosmos, which had been drawn up by Argelander. However, here are fifty of the principal Variables known to be periodical, including both the discoverer and the classifier of each:—

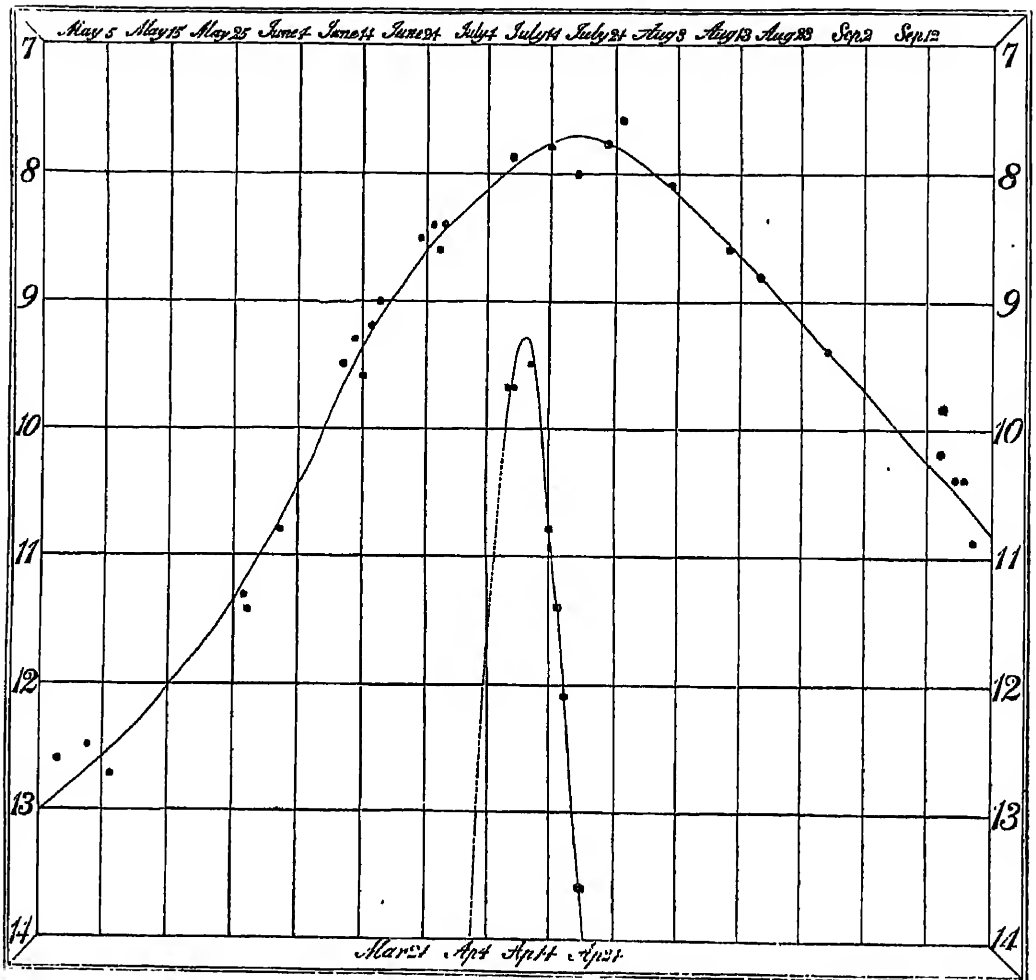
Star.	Discoverer.		Magnitudes.		Elements.		
	Name.	Date.	Max.	Min.	Epoch.	Period in Days.	Authority.
T Piscium	Luther	1855	9·5	11	1856 End of October	About 143	Schoenfeld
α Cassiopeiæ	Birt	1831	2	2·5	1844 Jan. 10	79·1	Argelander
S Piscium	Hind	1851	9	13	1851 August	About 13 months	Pogson
R Piscium	Hind	1850	7·5	9·5	1857 August	343	Schoenfeld
o Ceti (*Mira*)	Holwarda	1638	2	Under 12	1861 June 16·45	331·336	Argelander
β Persei (*Algol*)	Montanari	1669	2·3	4	1854 Oct. 8·2235	2·86727	Argelander
λ Tauri	Baxendell	1848	4	4·5	1859 Jan. 1·256	3·9520	Baxendell
R Tauri	Hind	1849	8	Under 13·5	1856 Feb. 2	330	Winnecke
R Orionis	Hind	1848	9	12·5	1857 April 18	378	Winnecke
α Orionis	Herschel	1836	1	1·5	1852 Nov. 26	196	Herschel
ζ Geminorum	Schmidt	1847	3·8	4·5	1857 Feb. 7·300	10·158	Argelander
R Geminorum	Hind	1848	7	11	1856 Dec. 21·3	369·725	Pogson
R Canis Minoris	Argelander	1854	8	. . .	1854 April 12	About 367	Pogson
S Canis Minoris	Hind	1856	8·5	Under 11	1856 Dec. 10	335	Schoenfeld
S Geminorum	Hind	1848	9	Under 13·5	1857 Oct. 15·0	294·07	Pogson
T Geminorum	Hind	1848	9·5	Under 13·5	1856 Nov. 2·6	288·02	Pogson
U Geminorum	Hind	1855	9	Under 13·5	1859 Feb. 20·5	96·98	Pogson
R Cancri	Schwerd	1829	6	Under 10	1857 Feb. 23	380	Argelander
S Cancri	Hind	1848	8	10·5	1854 June 22·471	9·484	Argelander
S Hydræ	Hind	1848	8·5	13·5	1857 Feb. 20	256	Schoenfeld
T Cancri	Hind	1850	9.5	12	1856 February	Unknown . .	Pogson
T Hydræ	Hind	1851	9.5	10·5	1855 Sep. 22	About 320 days	Winnecke
R Leonis	Koch	1782	5	10	1858 May 8	312·57	Pogson
R Ursæ Majoris	Pogson	1853	7·5	13	1857 May 22·6	301·90	Pogson
R Virginis	Harding	1809	6·5	Under 11	1856 Jan. 13·79	145·724	Argelander
S Ursæ Majoris	Pogson	1853	7	12	1857 April 21·2	222·65	Pogson
R Hydræ	Maraldi	1704	4	Under 10	1858 March 7	449·5	Baxendell
S Virginis	Hind	1852	5·5	11	1857 March 24·8	380·1	Pogson
S Serpentis	Harding	1828	8	Under 10	1857 April 5	367	Argelander
R Coronæ	Pigott	1795	6	. .	1854 June 15	323	Argelander
R Serpentis	Harding	1826	6·5	Under 10	1857 Sep. 29	359	Argelander
R Libræ	Pogson	1858	9	Under 13·5	1858 May 2	Unknown	Pogson
S Ophiuchi	Pogson	1854	9·3	Under 13·5	1857 June 25·1	229 37	Pogson
R Ophiuchi	Pogson	1853	8	Under 13·5	1857 July 13·2	304·60	Pogson
α Herculis	W. Herschel	1795	3	3·5	1856 May 18	66·33	Argelander
R Scuti	Pigott	1795	5	9	1857 Nov. 23	71·75	Argelander
β Lyræ	Goodricke	1784	3·4	4 3	1857 Feb. 6·079	12·9064	Argelander
R Aquilæ	Argelander	1855	6·5	. . .	1857 July 20	351·5	Winnecke
R Sagittarii	Pogson	1858	8	Under 13	1858 July 3	405	Pogson
R Cygni	Pogson	1852	8	Under 14	1857 March 2·4	416·72	Pogson
χ Cygni	G. Kirch	1687	5	Under 11	1857 Dec. 10	406·06	Argelander
η Aquilæ	Pigott	1784	3·6	4·4	1857 Feb. 6·606	7·1763	Argelander
24 Cephei (*Hevelii.*)	Pogson	1856	5	11	1807	Probably 73 years	Pogson
U Capricorni	Pogson	1857	11	Under 13·5	1857 August	About 15 months	Pogson
T Capricorni	Hind	1854	9	Under 14	1855 Oct. 25	274	Schoenfeld
δ Cephei (2nd)	Goodricke	1784	3·7	4·8	1857 Feb. 0·052	5.3664	Argelander
β Pegasi	Schmidt	1848	2	2·5	1858 Dec. 24	43·4	Auvers
R Pegasi	Hind	1848	8·5	Under 13·5	1855 Jan. 29	378	Argelander
R Aquarii	Harding	1810	7	Under 10	1857 April 16	388·5	Argelander
R Cassiopeiæ (2nd)	Pogson	1853	6	Under 14	1856 Nov. 24·3	434·81	Pogson

I must now observe of α Cassiopeæ, that, but for its having been patronized by such an observer as Argelander, I would have discarded it from the list; for, after gazing again and again at it under pretty close comparison, I can still hardly subscribe to there being any variation at all (see the Cycle, II. page 13): while two stars which had gained admission among the Variables—3 and 18 Leonis—are dismissed as "not proven;" the former showing but slight indications of quick changes, and the latter having been under rather regular inspection as a comparison star to R Leonis, without faltering. On the whole, some errors are suspected by mistaking 3 and 18 for the said R; but they have inflicted a deal of trouble in the examination of them. These intricate phenomena are indications of energetic activity in remote space, for which, as yet, no conclusive explanation can be given: still they appear to be the source towards which we must look for the occasional disappearance of some stars, the sudden apparition of new ones, and the advent of those designated temporary. Renewed inquiry into their details is imperatively called for: and it is a question whether they do not imply rapidity of change either in the position of the star's poles of rotation, or in the situation of the places passing through the orbits of opaque planets which may revolve around them. Zeal and watchfulness will be in demand, as these extraordinary bodies are subject to sensible irregularities in their successive phases, which as yet defy the application of fixed laws.

In February 1858, Mr. Pogson favoured me with the curves of a couple of variable stars—namely, his own R Ophiuchi, of *ordinary speed*, and U Geminorum,* a *rapid changer;* for it is subject to alternations or twinklings of light from the 9th to the 13th magnitude, in intervals of only from 9 to 15 seconds of time, the neighbouring stars of equal brightness remaining steady, and therefore the scintillations could not be owing to atmospheric disturbances; both these curves are projected on the same scale, and from them I had this diagram cut:—

* In a letter which I received from Mr. Pogson, 23rd November 1858, he says, "After twice missing its maxima, U Geminorum has suddenly burst forth, attained its fullest lustre, and again faded away; and that too within four days of my predicted time."

Projection of the observed Maximum of R. Ophiuchi, 1857, July 16.
Chief-Epoch, 1853, May 12.2. Period 304·60 days.



Projection of the observed Maximum of U. Geminorum, 1857, April 10. Magnitude 9·3.
Chief-Epoch, 1855, Dec. 14·8.
Period 96·08 days. Invisible about 80 days.

The above list enrols those variable stars which bedeck our Northern hemisphere, where Mira and Algol (Cycle, II. pages 59 and 78) still maintain their claims to thoughtful admiration. But we must just step across the equinoctial-line to introduce a most remarkable object, η Argus, to the patient reader, more on account of its surprizing fluctuations, however, than from any due demand, as yet, for its being classified as a periodical or established variable star. It is located in a spot which I often gazed at in days of yore, with a ship's "spy-glass," namely, that part of the Via Lactea which lies between the after-hull

of Argo Navis and the Southern Cross; in the midst of a large and brilliant nebula. This wonderful star appears to have continually varied in lustre ever since 1677, when Halley rated it of the fourth magnitude; in 1751, Lacaille entered it of the second lustre; and from 1811 to 1815 Burchell saw it reduced again to the fourth degree: but in 1827 the same observer found that it equalled the neighbouring first magnitude star, α Crucis, in effulgence. On the new year's day of 1845, my son Charles Piazzi Smyth wrote to me from the Cape of Good Hope, in a very enthusiastic way, bearing upon this same object—"The temporary increase of η Argus in March '43," said he, "so as to approach the magnitude of Canopus, gave reason to look forward to still greater brilliancy. But its maximum lasted only a few days in the early part of that month, after which it dwindled down rapidly. Hence, although the star may not be advancing by successive steps, it seems to do so by undulations; for in March '43 it did not go down to the point from which it had risen, and, being now on the increase, has been for a month brighter than Canopus, indeed half-way between it and Sirius, and is very red. These fluctuations in lustre probably form part of a cycle of longer period." In July 1850 my friend Lieutenant Gilliss, of the United States Navy, wrote to me from Santiago de Chili, in an interesting letter, which I communicated to the Astronomical Society (Monthly Notices, xi. pages 42-44), that he saw η Argus undoubtedly brighter than the joint light of the two components of α Centauri—which superb double-star is reckoned, "beyond all comparison, the most striking object of the kind in the heavens." Again, in Jan. 1852 (Monthly Notices, xii. pages 175-177)—

I have thought for the last eight or ten nights that η Argus is diminishing in brightness. It certainly is not brighter than the combined light of α Centauri, as it was again last summer; but the two are not yet favourably situated for comparison, being at unequal heights.

Can you not persuade one of your munificent patrons of astronomy to send his telescope out here? I am convinced that it would unfold more wonders in physical astronomy *every year* than a life would afford in your atmosphere.

By the triumph of moral over physical causes, however, this atmosphere maintains the advantage as yet. Moreover α has yielded its parallax, and shown itself, as at present known, by far the nearest to us of the "fixed stars."

In order that the surpassing beauty of α Centauri should be understood by philastrics, it must be recollected that A is of the first magnitude, B of the second, and the two are 16″ apart. Sir John Herschel describes them as being of a high ruddy or orange colour, "constituting together a star which to the naked eye is equal or somewhat superior to Arcturus in lustre:" but after his exordium, I certainly should have expected even greater brilliance. As a proven binary its orbit is a point of great interest; and it seems that the elliptic elements deduced by the zealous computers, though still susceptible of amendment, represent the observations pretty fairly. Having had one of these investigations referred to me by the Astronomical Council, I received the following valuable communication from Mr. Hind: and, finding the results assume as near an approximation to truth as the observations yet will admit of, I forwarded the letter to the Society, from whose Monthly Notices (xv. p. 88), it is here re-produced:—

I believe a paper upon the elements of α Centauri was referred to you for report yesterday. I have recently deduced an orbit for the same star, not being aware that any one else was engaged upon the same investigation. My results, in which I have some confidence, are founded upon all the micrometrical measures from 1834 onwards, including the excellent series by Captain Jacob down to the present year. I have thought you might like to have the elements for comparison with those given in the communication to the R.A.S., which I have not seen:—

Per. pass.	1859·42
☊	16° 42′
λ	26 2
Eccentricity	0·7752
Mean annual motion	+ 4·448°
γ	62° 53′
a	13·57″
Period	80·94 yrs.

These elements agree very well with the observations, so far as I have examined them.

Dec. 9, 1851.

To resume η Argus. Pondering over its interesting but perplexing photometrical changes, Sir John Herschel observes that—

A strange field of speculation is opened by this phenomenon. The temporary stars heretofore recorded, have all become totally extinct. Variable stars, so far as they have been carefully attended

to, have exhibited periodical alternations—in some degree, at least, regular—of splendour and comparative obscurity; but here we have a star fitfully variable to an astonishing extent, and whose fluctuations are spread over centuries, apparently in no settled period, and with no regularity of progression. What origin can we ascribe to these sudden flashes and relapses? What conclusions are we to draw as to the comfort or habitability of a system depending for its supply of light and heat on so uncertain a source? It is much to be regretted that we are without records of its changes in the intervals between the observations of Halley and Lacaille and those of Lacaille and Burchell. Its future career will be a subject of high physical interest. To this account I will only add, that, in the beginning of 1838, the brightness of this star was so great as materially to interfere with the observation of that part of the nebula surrounding it which is situated in its immediate vicinity, and, in particular, almost to obliterate that extremely curious oval or lacuna which forms so conspicuous a feature in the figure of the nebula annexed, and of which, had I not previously secured a correct representation, I should then scarcely have been able to have done so to my own satisfaction.

The accurate representation of this nebula, with its included stars, has proved a work of very great difficulty and labour, owing to its great extent, its complicated convolutions, and the multitude of stars scattered over it. To say that I have spent several months in the delineation of the nebula, the micrometrical measurement of the co-ordinates of the skeleton stars, the filling-in, mapping-down, and reading-off of the skeletons when prepared, the subsequent reduction and digestion into a catalogue, of the stars so determined, and the execution, final revision, and correction of the drawing and engraving, would, I am sure, be no exaggeration. Frequently, while working at the telescope on those skeletons, a sensation of despair would arise of ever being able to transfer to paper, with even tolerable correctness, their endless details.

And of this vast nebula, which probably is immeasurably beyond the fluctuating star in space, he eloquently adds:—

It would manifestly be impossible by verbal description to give any just idea of the capricious forms and irregular gradations of light affected by the different branches and appendages of this nebula. In this respect the figure must speak for itself. Nor is it easy for language to convey a full impression of the beauty and sublimity of the spectacle it offers when viewed in a sweep, ushered in as it is by so glorious and innumerable a procession of stars, to which it forms a sort of climax, justifying expressions which, though I find them written in my journal in the excitement of the moment, would be thought extravagant if transferred to these pages. In fact, it is impossible for any one with the least spark of astronomical enthusiasm about him to pass soberly in review, with a powerful telescope and in a fine night, that portion of the southern sky which is comprised between 6^h and 13^h of R A, and from 146° to 149° of N P D, such are the variety and interest of the objects he will encounter, and such the dazzling richness of the starry ground on which they are represented to his gaze.

THOUGHTS ON THE NEBULAR HYPOTHESIS (Cycle, I. pages 342-7, and II. passim). In my Catalogue I enrolled some of the finest nebulæ to be seen

in this hemisphere; not that the class I was addressing could discern all the wonders of those highly-attenuated, incandescent, self-luminous masses, but they were inserted that the gazer also might admire and adore. Indeed, whether these bodies—often so irregular, yet permanent in form—consist of a conglomeration of stars, or of cosmical vapoury matter, they are equally of vast importance to a better knowledge of the contents of illimitable space, and of the admirable structure of the Universe.

It was therefore that, after a considerable study of those objects, and following in the wake of such leaders as Tycho, Kepler, Halley, Lacaille, Herschel, and Laplace, I certainly had fallen into the condensation-opinion, an opinion which, although now somewhat shaken, I still hold to be conformable to probability and analogy. Yet, as all our conceptions of material objects are relative, and overlooking the point that the longest periods of the creative elaboration are but as a moment in the scale of eternity, I made the staid reservation that "Nature has yet to be caught in the fact" (Cycle, I. page 313); and I moreover expressly stated that the theme, though solemn and sublime, is yet only imaginative (Cycle, II. page 270). It was the remarkable harmony subsisting among their movements which induced Laplace to conceive that the sun, planets, and satellites of our own system had resulted from a common genetical process: he considered the rings of Saturn to be extant witnesses of the action of a general physical cause; and this opinion was advanced long before the nebulous semi-fluid dark ring had been detected.

More recent assumption inculcates that Lord Rosse's grand discoveries have proved all nebulæ—instead of being luminous uncondensed matter—to consist of immeasurably remote galaxies of suns, each like our own Via Lactea, the re-solvability of which is solely a question of power equal to the distance; but it is considerately admitted that the hypothesis was a "splendid vision."*

* A lesson to arrogance, in mighty problems, is found in the slight regard originally paid to the elder Herschel's experiments on the contested theory of the Solar Motion: and (in another branch of science), when a proposition to propel vessels by steam was made to our Admiralty, the Board complimented the ingenuity of the invention, but pronounced that "it was not adapted for the Royal Navy." Ohe!

The oppositionists, however, must not heave ahead too fast. Sir W. Herschel adopted the opinion only after re-solving hundreds of nebulæ into stars, insomuch that at first he thought all those objects might be re-solved under sufficient optical enlargement. Now, the late vast improvements in telescopic vision, though they have diminished, are far from having exhausted, the supposed number of actual nebulæ; and it is a very hasty conclusion to assume, because many of them have yielded, that no nebulous matter exists. Indeed, Lord Rosse expressly says, "that now, as has always been the case, an increase of instrumental power has added to the number of clusters at the expense of the nebulæ, properly so called; still it would be very unsafe to conclude that such will always be the case, and thence to draw the obvious inference, that all nebulosity is but the glare of stars too remote to be separated by the utmost power of our instruments." In a word it may be stated, as the general result of his lordship's examinations, that to whatever telescopic investigation these distant mysteries are submitted, nebulosity actually exists in different degrees of condensation: and, whenever they yield to the re-solvent power of instruments, a great number of stars are brought to view—discrete centres of condensation—or points surrounded with luminous matter. But clearly this does not include the fantastic wisps, and diffuse patches, of light which stud the celestial regions; and, as to those strange problematical objects—the planetary nebulæ—what cosmogonist can examine them without intense amazement, and utter incapacity of conception?

The Nebular Hypothesis was never advanced as a positive inductive fact, or physical reality, but only a philosophical conjecture, or an assumed analogy; yet, if there ever did exist a reason for accepting the speculation as a *vera causa*, that reason, though impinged upon by the recent telescopic revealments, is not entirely swamped thereby; on the contrary, the celestial scheme is equally remarkable and suggestive, nor can it be ignored without first proving, that nebular condensation has never been going on as indicated by the internal evidence of our own system. It is a powerful incentive to obtain by further research demonstrative facts, and consequent truth; so that astronomers in

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after-times may become sensible of any changes which may take place in the "island universes" scattered in the depths of space, which, from their enormous distance, can only become known after some ages shall have passed away. The Nebular Hypothesis is therefore worthy of being retained; it is not what Bacon termed "the seduction of a vain philosophy," but a fair inference from second causes, wherein the consequences conform approximatively to the phenomena of our system, and may afford us a faint conception of an OMNIPOTENT FIRST CAUSE, though no idea of His Essence or Will.

TYPICAL ERRORS (Cycle, I. page 398, line 13 *ab imo*). The mean determination of the level should be 0′ 0″ ·05, instead of 0′ 0″ ·5. On page 400, line 15, for 40 abatement from the index, read 20.

ON OBSERVING STARS BY DAYLIGHT (Cycle, I. page 401). In stating the claims of Gautier and Hook to have originated this practice, it appears that I overlooked an anterior title to the discovery. After that volume was published, I received a friendly letter from Professor David Thomson, dated "King's College, Aberdeen, 18th January, 1847," to this effect:—

Permit me to call your attention to the following quotation from Galileo's 1st Dialogue on the Copernican System (Opere, Milano, 1808, &c. Vol. XI. p. 206), which will afford a correction of a statement made at p. 401 of your excellent Prolegomena:—

"Se voi paragonerete il piccolo corpicello, v.g. della Canicola, *veduto di giorno* col Telescopio, quando si vede senza irradiazione, col medesimo veduto di notte coll' occhi olibero, voi fuor di ogni dubbio comprenderete, &c."

This was written before 1632, that is, 37 years before Gautier's and Hook's discovery of the visibility of certain stars by day with the telescope."

LEÇONS D' ASTRONOMIE (Cycle, I. page 452-6). This was a pamphlet that suddenly appeared and drew attention, but, as it professed to tread on ground which I had been beating, I re-produced its constellations. Still, I mentioned my surprise that M. Arago should have been the author of such a work, and that its only claim to notice was its bearing his name. Since

my book was published, he has denied the authorship "with both hands." He produced a work of his own, from a course of Lectures on Astronomy which he was in the habit of delivering annually at the Observatory of Paris during a period of 18 consecutive years, and this work—Popular Astronomy—has been translated by Mr. Robert Grant and myself. A glance will prove that the two publications cannot be by the same author.

(Cycle, I. page 436.) At line 12 *ab imo*, instead of the typical error *au coch* read *un coch*.

An erroneous Statement (Cycle, I. page 457). In the last paragraph on this page is a great oversight, in stating the effects of the shifting of the node of the Earth's equator; for if anything is more certain than another in this department, it is that the latitudes of the stars are not changed by the precession of the equinoxes, nutation, or any other branch of that phenomenon which depends on the spheroidal figure of the Earth. These latitudes, it is true, are subject to an exceedingly slow and minute change, but that is occasioned by the alteration of the plane of the Earth's orbit, due to the attraction of the planets, and would be precisely the same if the Earth were a perfect sphere. The paragraph should therefore be read as follows:—

First in importance is that continual shifting of the node of the Earth's equator on the ecliptic, by which the line of reference on the ecliptic actually varies, altering equally all the longitudes of the stellar host. This singular variation, as already shown, has long been recognised as the precession of the equinoxes. Were the position of the Earth's axis always accurately parallel to itself, the line of reference, which points to the first point of axis in the heavens, would also remain parallel to itself; but this is not the case, the actual plane of the ecliptic does not vary perceptibly by the action of the Moon on the Earth, or rather the very slight oscillation which it undergoes is one of the smallest of astronomical phenomena; but, by the action of the planets, it gradually diminishes, very slowly, and this is the only phenomenon by which the latitudes of stars are affected. Newton, to whose sagacity we are indebted for the discovery of the cause of precession, supposes the spheroidal form of the Earth and Moon to be owing to their having once been in a fluid state, whence, as a consequence of the rotation, the matter flowed most to the equatorial regions.

Before quitting the first volume of the Cycle, we have occasion to return to its page 64, where it is mentioned that Pius VII. had "repealed the edicts

against the Copernican system," and that, "notwithstanding which various publications still adhered to the Earth's immobility." This was a patent affair in 1818, and made, I well remember, a considerable stir at the time. It was communicated to me either by Baron de Zach or General Visconti; but the reference to the subject has been lost, or mislaid, in the *gurgite vasto* of my Mediterranean papers. Therefore on Dr. Whewell's lately wishing me to get him a copy of the rescript, I immediately wrote to a highly scientific Italian friend for exact information on the subject, and received by return of post the following reply, dated *Roma*, 10 *Genn*°, 1857:—

I now come to answer your last note, in which you request to know something of what Pius VII. did relative to Galileo. All I know is only this, that he ordered in the reprint of the Index of Prohibited Books, that, as already had been done some time before, the law prohibiting books on the Motion of the Earth should be omitted.

This does not mean that there is any change in the Catholic *Belief*, since the prohibition of a book is not always on account of its being erroneous, or heretical, but because the discussion of certain topics may be dangerous at certain times, though they may be permitted when that danger has passed away. This has occurred even with some very good theological books, prohibited only because they might lead to the treating of matters and questions which were perilous owing to the times. These, however, were articles of discipline which might vary in process of time; and often the element of human miseries may be mixed up with them, as much on the one side as the other.

Galileo certainly had a long tongue, and the men with whom he contended were not to be assaulted roughly. If he had followed the Grand Duke's advice, and that of other friends, he would not have suffered what happened to him.*

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* The whole labours of such men as Galileo, Newton, Boyle, Descartes, Bacon, Riccioli, and Mercator, were not damned, but only selected portions of them. In the instance before us it was the *Dialogo sopra i due Sistimi del Mondo*, which was anathematised and burned at Rome by a decree of April 1634; such writings as those of Luther, Zwingle, Bruno, Wickliffe, and many others were "omnino damnantur." As my correspondent's letter is of some import, it may be proper to submit the original:—"Vengo ora alla risposta dell' ultima sua, nella quale mi chiede di sapere qualche cosa sull' operato di Pio VII. intorno a Galileo. Quello che io sò è solamente questo, che il detto Papa ordinò che nella ristampa dell' Indice dei libri proibiti non vi si mettesse più—come pure alcun tempo prima era già stata levata—la regola generale che proibiva i libri che trattavano del moto della terra.

"Questo non vuol dire che siasi mutato nulla nel *Credo* Cattolico, perchè è ben noto che un libro non è sempre proibito perchè erroneo, o eretico, ma perchè può esser pericoloso in certi momenti e tempi il trattare alcune materie, cessato il quale pericolo i libri possono permettersi. Ne abbiamo esempj in casi di sanissimi libri teologici, proibiti solo perchè potevano dar causa a trattare materie e quistioni pericolose per le circostanze dei

Such is the substance of my worthy correspondent's communication, the which does not altogether quadrate with my own impression of the matter. The mere omission of books from the *Index Expurgatorius*, or expunging a name, certainly does not prove much; and it is pretty clear that one Padre Sanctissimo would hardly be brought to reverse the sentence of his predecessors: by the way, the late restoration of the Jesuits was a step in the opposite direction.

As faith and science ought never to be pitted against each other, we must have a word more *sulla condanna del Galileo*. From Tiraboschi to Marini the great Tuscan philosopher has been rapped on the knuckles as being unjustifiably fidgetty under interference. Nor is it only Catholics and Catholic prelates who assail him, but even my judicious and very temperate friend De Morgan, in 1855, thus lays it on:—

It must also be borne in mind that the Inquisition did not interfere until both sides had got into a warm and angry dispute about certain interpretations of the Scriptures, of which both sides admitted every interpretation to be the exclusive property of the Church. From many circumstances we feel a right to suspect that if Galileo and his followers had never made any other answer to the Scriptural argument except declining to assume the power which, by their own concession, belonged only to popes and councils, they never would have been called to account. The general temper of the higher orders of the clergy seems to have been unobjectionable. When Galileo applied to Maraffi, the general of the Dominicans, complaining of an indecent attack made upon him by one of the order, he received a written answer, expressing regret and the vexation of the writer at being liable to be compromised by all the brutalities (*bestialità*) which might and did take place among twenty or thirty thousand monks. Had each offender against common sense and common modesty been a pope, a cardinal, an abbot, or a monk, acting in his single capacity, we should have been pleased to remember how vast and how many have been the obligations which both literature and science have owed to those orders. And we should have recalled the great encouragement which the Copernican system received from popes and cardinals at its first promulgation; nor should we have forgotten that immediately after the first proceedings against Galileo, a pope invited Kepler, the greatest and most notorious of the Copernicans next after Galileo, to be his professor of Astronomy at Bologna. But the Inquisition, the common sewer of the *odium theologicum*, has no redeeming point in its general history, though the case before us has

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tempi. Del resto, queste sono cose disciplinarie che variare possono col tempo; e spesso vi si può mescolare l'elemento delle miserie umane, tanto da una parte che dall' altra.

"Galileo certo ebbe la lingua non poco lunga, e quelli con cui contrastava non era gente da prendere di fronte; e se avesse seguito i consigli del Gran-duca, e d'altri suoi amici, non avrebbe sofferto quello che gli toccò."

this much of alleviation, that (taking for granted that Galileo was *not* put to the torture) there is not enough of atrocity to take off the edge of the ridicule.

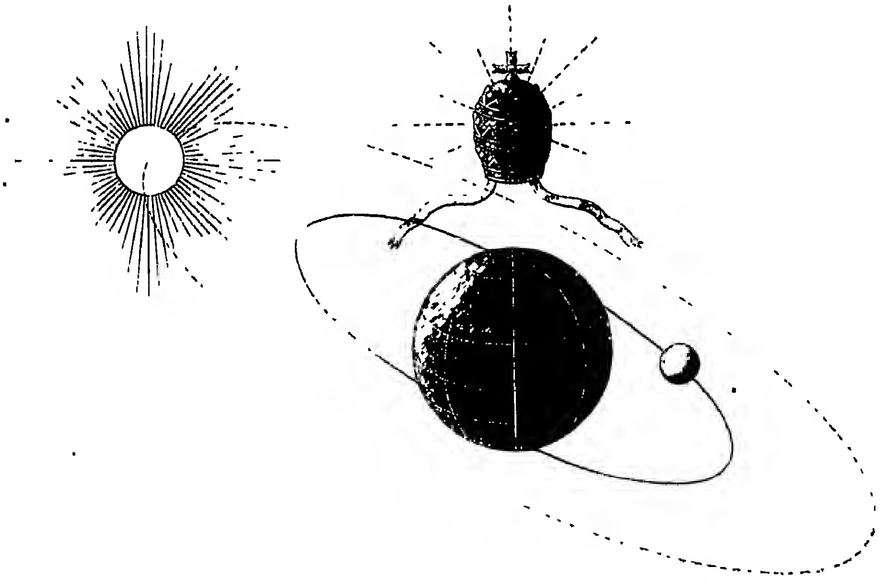
Now, without denying this, I may say that few of those writers, happily, have known what it is to be cited before the Holy Office of the Inquisition, then and there to be questioned and cross-questioned by stern and merciless Dominicans, armed with power to squeeze out answers as to their belief in these matters. Nor were those persecutors who dragged the venerable philosopher through the streets in a penitential dress, his only enemies. But some, taking their seats between sanctity and science, have paid the penalty by the laws of physics in such case made and provided. Among them a Neapolitan, Julius Cæsar La Galla, really a man of information, was a violent antagonist of Galileo; and he, in a book dedicated to Cardinal Capponi in 1611, recklessly asserts that, because God is not on earth but in heaven, he therefore can move heaven but not the earth, "Iddio non sta in terra, ma in cielo, onde può movere il cielo e non la terra;" and such asinine balderdash was allowed, among the Anti-Galileans, to pass as argument!

At the mention (Cycle, I. page 46) of Galileo's imprisonment for a year, it alludes to the close incarceration to which he had been condemned for life, and the *liberation* only to his becoming a prisoner-at-large, in which state of thraldom he passed the rest of his days, even after total blindness afflicted him: instead of liberation, it were better to have said, mitigation of his sentence. It was in 1634 that he was permitted to reside in his casino at Arcetri, near Florence; but the humiliation pressed heavily on his spirits, and increased his infirmities, insomuch that he never more wrote on Astronomy. Milton, afterwards his companion in the *Index Expurgatorius*, called upon him in 1638. "I found," says that sublime poet (Prose Works, i. page 313), "I found and visited the famous Galileo, grown old, a prisoner to the Inquisition for thinking on astronomy otherwise than as the Dominican and Franciscan licensers thought." And, in his immortal poem, with a lively recollection of the astronomer's first application of the telescope, he paints Satan—"of unblest feet" with his ponderous shield—

Hung on his shoulders like the Moon, whose orb
Through optic glass the Tuscan artist views
At evening from the top of Fesolé,
Or in Valdarno, to descry new lands,
Rivers, or mountains, in her spotty globe.

In reading which it should be recollected that, in Milton's time, the designation "artist" implied a man of science; and in the nautical affairs of that day, the mariner who could write a journal, keep a sea-reckoning, or shoot the sun for a latitude, was the ship's Artist.

To conclude—such was the deplorable doom of a philosopher to whom astronomy, mathematics, geometry, mechanics, acoustics, harmonics, and the general knowledge of nature are under such durable obligations! After all, it seems that we are still to rest on our oars upon the main question, although *E pur si muove* is so unequivocally manifest; for, what with open foes, and our own traitorous Dominicans in disguise, among many the glorious sun must still profess to run round the terrestrial globe:—



CHAPTER II.

ORIGIN OF THE HARTWELL OBSERVATORY.

> What is a man,
> If his chief good, and market of his time,
> Be but to sleep and feed? a beast—no more.
> Sure, He that made us with such large discourse,
> Looking before, and after, gave us not
> That capability and godlike reason
> To fust in us unused.
>
> SHAKSPEARE.

FREQUENTLY passing through Bedford to his seat at Colworth, and attending the local sessions and assizes as a county magistrate, Dr. Lee was wont to favour me with occasional visits; and, having always been an ardent admirer of Urania, he felt the enjoyment of a little practice, as well as the advantage of a good telescope, easy to manage, and always ready for use. I do not accuse him of violating the tenth commandment; but it was clear that he would not remain much longer without seeking some powerful means of penetrating space. And thus it happened.

In December 1828, soon after I had completed my observatory at Bedford, and mounted the instruments lent by the Astronomical Society for that purpose, it was communicated to me that the telescopes, clock, transit circle, portable transit, and numerous other articles, which had belonged to the late Rev. Lewis Evans, were to be disposed of by private sale. On viewing them, I was rather chagrined at the circumstance not having occurred before my arrangements were carried into effect; especially as the circle seemed to

me greatly superior in simplicity and efficiency to Colonel Beaufoy's, with which I had just commenced my operations (See the Cycle of Celestial Objects, vol. i. pp. 333 and 335). On mentioning this to Dr. Lee in the evening, he resolved to make the purchase, and to present the circle to the Astronomical Society, under an understanding that it was to change places with the one at Bedford; a transaction which accordingly took effect.

This beautiful transit-circle being thus disposed of, there still remained a considerable number of instruments with Dr. Lee, of which he shortly afterwards became desirous of making a proper use. On being consulted, I recommended that the small transit-instrument should be mounted on a pedestal in the South portico of Hartwell House, where it would command all the Greenwich stars from the zenith nearly to the horizon—that is, from γ Draconis down to Fomalhaut; the adjoining strong-room then could be fitted for the clock and other instruments, and the requisite books and catalogues: while a second stone pier was proposed to be erected at a little distance in front of the portico, on which to place the reflecting-telescope. This proposition was adopted, and, with the help of Dr. Lee's staff of domestic artisans, I soon carried it into effect, insomuch that, having approximated the meridian pretty closely, the moon and several stars were observed on the 3rd of April, 1830.

A word upon this point, as I think it wholesome in affairs of such description, that things should go by their right names, to prevent misunderstanding and wrong impressions; for I have witnessed with very deep regret the *vis inertiæ* perceptible in certain quarters of the Amateur World, wherein zeal so frequently evaporates in the note of preparation. While certain staid Philasters voluntarily engage themselves in systematic labour for the benefit of Science, it is notorious that there are several private well-built and well-equipped observatories among us wherein no useful observations, or matters worthy of record, are made or undertaken to be made, save when a publicly notified phenomenon is to be seen. To be sure, a well-mounted telescope ought to be an appropriate and desired article for every educated gentleman's establishment, both for amusement and expansion of mind; and the Earth

noiselessly rolling on its axis presents a new field for admiration with every passing hour. What scenes in the diurnal revolution of the celestial sphere, in the 86,400 seconds from the time of a star's passing the middle wire to its return to the same point! The whole is a marvellous moving panorama, one day pressing on the heels of another, and the moon under continual change:—

> Truditur dies die,
> Novæque pergunt interire Lunæ.

But this, so much better than neglect, has no more to do with the desiderata of Astronomy, than swimming has with the composition of water. Instead, therefore, of bruiting and advertising their instrument-rooms as observatories, it were better they should recur to the more honest and correct term *gazebo*. Not but there are those lovers of Urania, a zealous band, who have resolutely climbed the cliffs of knowledge to enter the Temple of Science by its only true approach; and have rendered such valuable and essential service to the cause, that they ought never to be confounded with the gazers alluded to. By the accurate advances of this force, observations are furnished for the discussion of the geometer, and the generalisation of the philosopher; and it is a course which may elevate the amateur to the platform of science, without lowering science to the level of the amateur. For more on this topic, I beg to refer the "gentle" reader to the Cycle, vol. I. pages 366-9, and the present paragraph may be concluded with old Joseph Walker's remarks on the gratification which a telescope affords. In his Astronomy's Advancement, or News for the Curious, 1684, he says, "Any person may be able to direct himself to what stars are in a most convenient condition for him to view at any time, when his curiosity leads him to such entertainments. And truly these are entertainments so noble and glorious, as well as ravishing and transporting, that it is to be wondered how persons whose parts and fortunes qualifie them for them, are able to temperate themselves from them." From obvious causes, however, there will always be more investigators and speculators than observers;—since, in the latter class, bodily comfort, physical ease, and the seductions of rest, must be cast adrift. To return—

I should here mention that the transit-instrument above-mentioned was a portable one, of twenty-four inches in length, on a cast-iron stand, by Carey;* and the reflecting telescope is of five inches and a half aperture, with a focal length of thirty-six inches, having a good finder and several eye-pieces, on a stout brass tripod. The clock was made by Mr. R. K. Barton, of Ramsbury in Wiltshire, and exhibits a pretty fair train of wheel-work to a Graham's dead-beat escapement. It is fitted with a novel pendulum, expressly made for the Rev. Lewis Evans, of Froxfield, by his friend Edward Troughton, who had just invented it. In the construction of this ingenious application, the apparent rod is a cylindrical tube of brass, reaching from the bob to the suspension-spring. This contains another tube with five wires inside it, so disposed as to produce three expansions of steel downwards, and two of brass upwards. These lines of wire are so proportioned as to length, that they act inversely upon each other's impulses, and by the combination destroy the effect that either metal would have singly. In this contrivance the estimable artist reasoned well, and the attempt was worthy of him: but it so happens that the main variations by which the instrument is affected are owing to the outer or covering tube, and, even were this removed, it would still be liable to the jumps and irregularities incident to gridiron pendulums. Troughton therefore soon abandoned it as a failure.

The aforesaid strong-room, besides being the repository of the fixed instruments, contained also various portable ones, and presented sufficient means for breaking-in an amateur of zeal, and also for doing much good work, terrestial as well as celestial. Of these I may instance the following:—

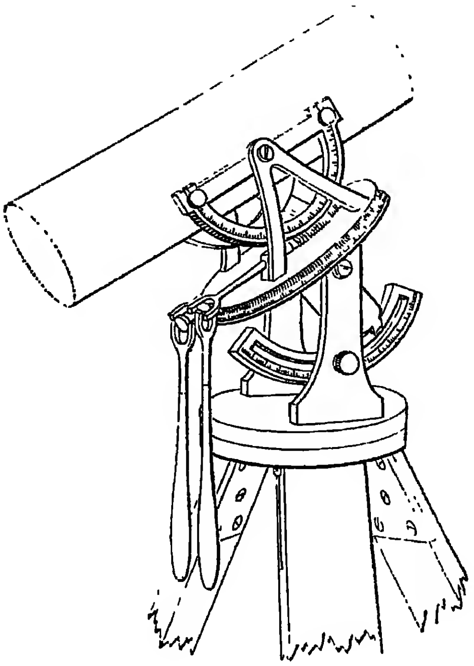
A reflecting circle by Troughton, with counterpoised stand, and an artificial horizon.
A ten-inch brass sextant, by Ramsden.
A five-inch sextant, by Ramsden.

* This transit-instrument was afterwards lent to the Euphrates Expedition under Colonel Chesney, and went to the bottom of the river in the ill-fated iron steamer which foundered there in a heavy squall. It was, however, shortly afterwards recovered, and restored to Dr. Lee, who thereupon lent it to Mr. Lassell, of Liverpool, as an aid to his mighty equatoreal reflector.

A quadrant of eighteen inches radius, by Nairne, with diagonal graduation.
A quadrant of twelve inches, by Liverton, of Liverpool.
A trough, framed glass, and Mercury bottle for artificial horizon.
A seven-inch theodolite, and tripod stand, by Carey.
A small azimuth compass, in a brass box.
A ten-inch surveying compass, with sight vanes and a tripod.
A surveying spirit-level.
A clinometer of six inches radius.
A circular protractor of six inches in diameter.

Hence it will be seen that the little observatory was richly furnished; and it had, moreover, an abundance of lenses well-ground, coloured glasses, barometers, thermometers, and hygrometers. There were two or three telescopes, of which the Gregorian already mentioned was the best; but an old one deserves notice for its respectable age, and its tolerable performance: it is a vellum eight-drawer "spy-glass" of ten feet in length, having a single object-glass two inches in diameter, made by the celebrated Giuseppe Campani, of Rome—who, with his brother, Alimensis, constructed several gigantic telescopes for Louis XIV. Besides which, in the winter of 1829, when I became possessed of Sir James South's six-inch object-glass, I transferred to Dr. Lee a fine five-foot telescope of three inches and three quarters clear aperture, which had been specially constructed for me by the elder Tulley; and which proves to be one of the very best of its order: in fact, he had made several clever trials before producing this, as he said he was "well aware of what I required." This instrument is powerfully fitted with a range of eye-pieces varying from twenty to eight hundred times,* coloured glasses, sliding adapters, prisms, and micrometer; and, being mounted on a stout equatoreal stand, with rack-work motion and all necessary appliances, it formed a powerful addition to the means in hand. The solidity and efficiency of the portable mounting entitle it to notice—

* In mentioning the whole range of eye-pieces, I should remind a beginner that the working ones are those which magnify from seventy-four to two hundred and sixty-two times, as severally marked on them. The higher powers are merely for extreme experiment.



From having made numerous suggestions to ensure its full efficiency I must be allowed to dwell a moment longer on this beautiful instrument; especially since—for the manner of its equipment—it has obtained the designation of *Smythian*. From the use which I made of the double-image micrometer lent me by Dr. Pearson (Cycle, I. page 385), I was satisfied that the curious principle of rock-crystal prisms was eminently adapted for the micrometrical measurements in practical astronomy, and for certain delicate requirements the most desirable method of obtaining both angles of position and distances, under the conditions mentioned on page 386 of the Cycle. Its utility for officers on foreign stations struck me particularly, since the process requires no artificial illumination, nor indeed any adventitious light after the adjustment of the vernier line to the zero of the graduation is made; and, besides the avoidance of the lamp-trimming nuisance, there is impunity from the danger of rupturing spider-lines. Therefore in the year 1826, as there was a prospect of my being sent abroad, I concerted with the late ingenious artist Mr. Robinson, of Devonshire Street, to cut me a series of prisms, subtending

constant angles as arranged: these were afterwards accurately determined, together with the magnifying powers of a variable eye-piece by a Dollond's dynameter. The constant angles of these crystals (2ϕ) adopted from the number engraved on each prism-cap are—

Prism 1	$2\,\phi = 192''$	$\log.\,\phi = 1{\cdot}9823$
Prism 2	$2\,\phi = 386$	$\log.\,\phi = 2{\cdot}2856$
Prism 3	$2\,\phi = 554$	$\log.\,\phi = 2{\cdot}4425$
Prism 4	$2\,\phi = 1027$	$\log.\,\phi = 2{\cdot}7105$
Prism 5	$2\,\phi = 1596$	$\log.\,\phi = 2{\cdot}9020$
Prism 6	$2\,\phi = 2474$	$\log.\,\phi = 3{\cdot}0924$

which commanded a range from the closest wedge-shaped double-stars to wide pairs, and formed a perfect Ocular Crystal Micrometer. In practice the results are so easily obtained, and are so satisfactory, that I strongly urged the use of them to certain nomadic astronomers with portable instruments; but I regret—for their own sakes—that my recommendation was not attended to —*ma vedremo!* It is certainly a method of measuring which nothing but my possessing a large and stable equatoreally-mounted telescope, fitted with an efficient and full-powered double-wire micrometer, would have prevented my exclusively adopting.

In 1857, this instrument was lent to Mr. Norman Pogson, then of the Radcliffe Observatory, to use in the hours he was able to glean from official duties; and he perceived the use of the prisms, and managed the mode so promptly, that in the following year he communicated a descriptive account to the British Association for the Advancement of Knowledge, together with the measurements of various double-stars of interest. And the high character I have given to the telescope was fully borne out by his quickly finding one of the smallest planets—Hestia, with it. Writing to me on the 4th of August, 1857, enclosing some neat observations of the planetoid Victoria, he observes, "The Smythian telescope will be of great service in watching the variable stars, as it defines small stars so admirably. The companion of Polaris, and other more difficult tests, stand out in a clear sharp style which puts to shame certain larger object-glasses I have been in the habit of using." And see the

neat measures of Hestia on the 16th and 17th of that month, in the Monthly Notices of the Royal Astronomical Society (xviii. page 15).

Such was the first observatory at Hartwell, which, small as it was, created a desire in Mr. John Dell, of Walton, near Aylesbury, author of the volume of Evening Amusements for 1832, to possess also a clock and a transit-room; and I accordingly, at the request of Dr. Lee, superintended the erection and equipment of one for him. The two establishments were to be worked in emulation of each other; but, scarcely had the good Doctor conquered the difficulty of watching the stars across the wires while transiting, than he yearned for more power, and consequently a larger sphere of utility in the Uranian cause. His Alma Mater had instilled the physical theory of astronomy into his mind, and practice brought the conviction of its, so to say, tangible advantages :—

While thus we penetrate ethereal space,
    And Heav'n's wide expanse so minutely scan,
God's wisdom, pow'r, and handiwork we trace—
    The noblest study of aspiring Man.
New systems open to us as we climb;
    Each glittering star gives law to circling spheres,
Which run eternal rounds in faithful time,
    Nor err one moment in ten thousand years!
Perpetual motion Heav'n's high works maintain,
So often sought on earth, but ever sought in vain.

Besides contemplating the admirable balance and beautiful arrangements of the starry firmament, he now perceived the harmonious connection between the refinements of science and the wants of every-day life; and he hoped that the labour he chose to bestow upon astronomy might, while gratifying himself, be of some practical use in the end. I had the honour of being therefore again consulted upon the subject, and readily yielded my aid on the occasion.

Before, however, entering the new Transit Room, in giving an idea of the future prospects of this observatory, it may be as well to state at once, that, as I bade adieu to my voluntary labours in practical astronomy at the close of 1858— not from any flagging interest in the cause, but on account of entering my seventy-second year—it became a serious object that such an establishment as

the Hartwell Observatory should not be idle. As I had personally witnessed the earnest zeal and talent of Mr. Norman Pogson, it struck me that he would be a proper acquisition for the purpose; and, after a consultation with Dr. Lee, I obtained his permission to move in the affair. On this I directly applied to my late regretted friend Johnson, Director of the Radcliffe Observatory; and, finding him not averse to what he considered would be advantageous for his assistant, proposals were made—through him—to Mr. Pogson, and accepted. Accordingly this gentleman entered upon his new duties on the 1st of January 1859, under the following stipulations:—

I. Mr. Pogson will take the observatory, the instruments, and the appurtenances into his charge, together with the astronomical, mathematical, and scientific books, documents, and papers in the library. The Smythian achromatic ought to remain in Mr. Pogson's house (with the Barton clock) for occasional use when the larger telescope is not requisite—the observations carried on there to belong to Dr. Lee equally with those made in the observatory. He will occupy himself *generally* with keeping a fair transit-clock rate, and looking to remarkable occultations, eclipses, comets, and other casual phenomena; but in *particular*, with the following:—

II. Among the desiderata of Sidereal Astronomy, and a capital branch for practical scrutiny, is the inquiry into the condition and details of those wondrous bodies the VARIABLE, or more properly the PERIODIC, STARS. Though these have been occasionally observed from the days of Fabricius to the present, they have not yet received that undivided attention—especially in England—which, as a means of reaping grand results, they call for. This most desirable pursuit is therefore to constitute the first and principal duty of the Hartwell Observatory, which may be said to have already enlisted R Libræ, R Sagittarii, and that very curious star U Geminorum.

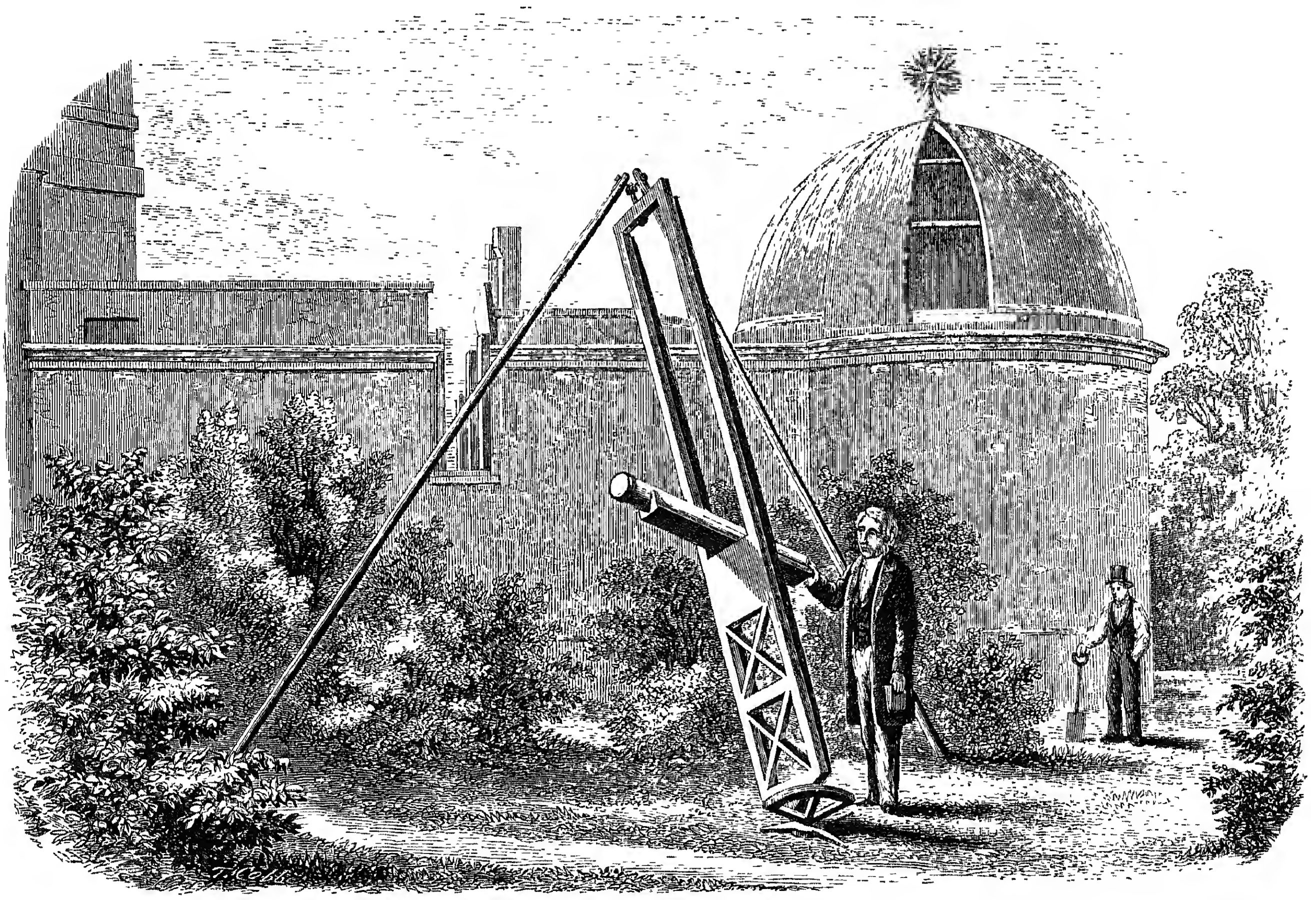
III. Planetoids, or Asteroids. A search for more of these very interesting bodies—of which Hestia already claims Hartwell for continuous notice—is also a great object for the large telescope, since from the recent discoveries they must now be sought for among stars of the faintest magnitudes: but this work may almost be included under article II., especially if Mr. Pogson completes his important

IV. Sidereal Charts, as they connect those of the Regent's Park Observatory and the Berlin Academy; and contain all stars down to the 12th magnitude. This is severe work for the eyes, but as it can only be advantageously pursued during the last and first quarters of the moon, the intervals will admit of attention being given to another branch of the highest sidereal interest, namely, the

V. Double Stars, of which those in the "Cycle of Celestial Objects" known to be binary, or suspected of being so, may be watched from time to time; especially as many of them were re-measured at Hartwell. Yet, No. II. of these terms being the intended main guerdon of the establishment, this kind of observation may be confined to the stars in that Catalogue—the primary of each system having been registered at Palermo, by the Abbate Piazzi.

VI. In the course of the labours here instanced, Mr. Pogson can use his own discretion in the instrumental means under his charge, and as to the form of his reductions: but as γ Virginis has been observed so long with the Hartwell equatoreal and wire-micrometer, it is advisable to repeat such measures at each apparition, for comparisons *inter se*, even if the rock-crystal prisms are also applied. And all the results and documents of every description relative to the observatory work, are to remain the property of Dr. Lee.

These conditions were duly signed under my cognisance: and the importance of the appointment is as unquestionable, as the looming harvest is desirable. Such being the agreement, we may close the present chapter with a tail-piece representing the outside view, from the south, of the observatory, of which Mr. Pogson thus assumed the command.



CHAPTER III.

THE TRANSIT ROOM AND INSTRUMENT.

Chaldean Shepherds, ranging trackless fields,
Beneath the concave of unclouded skies
Spread like a sea, in boundless solitude,
Look'd on the Polar Star, as on a guide.
Wordsworth.

From the sloping nature of the ground on which Hartwell House stands, and the mass of trees in the south and west quarters, the site was not very promising for an observatory of general capacity. After much examination, I at last recommended that a transit-room should be built at the south-east angle of the mansion, where a meridian could be commanded at all times, from the lower passage of Capella on the north, to the southing of Fomalhaut: and in this I proposed that the moon and moon-culminating stars should be observed as regularly as circumstances would admit of, by means of a well-mounted five-foot transit telescope; albeit, but for such an exigence, I am averse to seeing so large a meridian-instrument in the hands of an amateur.

There was another advantage attending the choice of this spot, namely, that, by converting one of the mansion windows into a door-way, the library and the observatory would be conjoined. This is no small luxury for a private establishment, where there is no necessity for constant application, or continuous work—like that of turning a cutler's wheel. It often happens that there are intervals between the observations on the agenda for the evening; and frequently as well as suddenly the weather varies for a time, with a

promise of clearing off again. Now in such cases here would be a desirable and pleasing retreat from the instruments into "The House of Treasures," as Ezra happily termed the depository of books and documents. Herein are found the solid productions of philosophers and men of sound acquirements in all the scientific branches; works so especially dear to those who observe the golden rule, of asking no questions on those points which they can find out for themselves, and thereby nail the memory. This is the true use of books, those silent friends, of the value of which Daniel Heinsius *(cited by poor Burton)* said—"I no sooner come into the library, but I bolt the door to me, excluding Lust, Ambition, Avarice, and all such vices, whose nurse is Idlenesse the mother of Ignorance, and Melancholy her self; and in the very lap of Eternity, amongst so many divine souls, I take my seat, with so lofty a spirit and sweet content, that I pitty all our great ones, and rich men, that know not this happinesse:" and Cowley, with a like feeling, exclaimed—

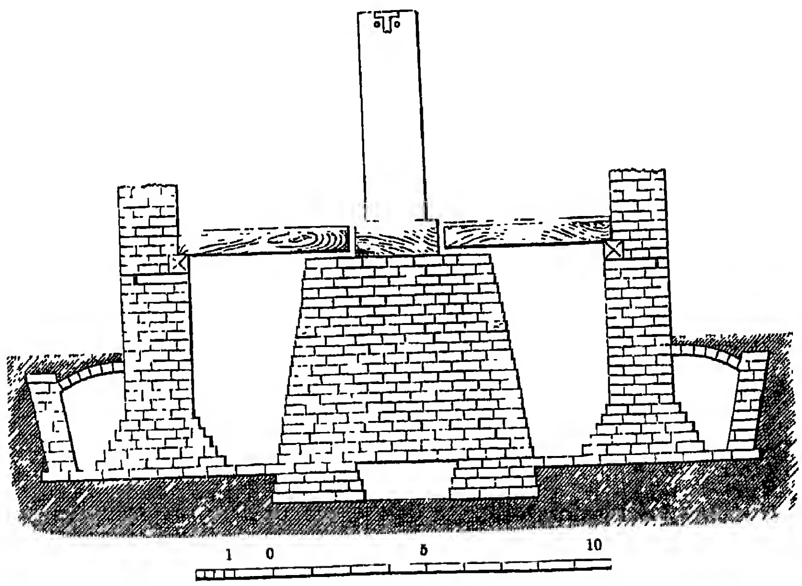
> Hail bank of all past ages! where books lie
> T' enrich with interest posterity!

The next point was, so to apportion the new structure, that it should deform the aspect of the mansion as little as possible: on communicating my anxiety on this point to my friend* Mr. Bevan, of Leighton Buzzard, he declared that it would never look half so ugly as the green-houses which he had seen against some of our best mansions; "and, after all," added he, "who would listen to any one who could dare to decry building an observatory?"

Early in 1831 I opened the trenches by drawing a meridian-line: my data were equal altitudes of the sun, taken with a well-adjusted reflecting-circle by Troughton, and an artificial horizon, and there was also the time by the portable transit. From this we raised perpendiculars to strike the angles of the south-east corner of the library wall, and, having stumped out the dimen-

* The late B. Bevan, C.E. died on the 1st of July, 1833. The night of the lunar eclipse was his last: he rose from his bed and observed the commencement, but expired during the progress of the phenomenon.

sions, dug through the sandy rubble down to the "live rock" of blueish compact limestone, for placing the foundation upon. As we had but the single instrument in view, I was resolved that it should experience no derangement or tremors for want of solid supports, and therefore had a large mass of the best marle bricks joined with cement raised for placing the piers upon, with a good space between it and the walls for ventilation; every care being taken to guard against damp, and a capacious dry drain laid around the building. The stone piers, each six feet high and cut from a single block of Portland stone, were then erected, and the flooring-planks were carried so as not to touch them, while end-play was freely allowed to the tread of people. This is the cross-section, from north to south :—

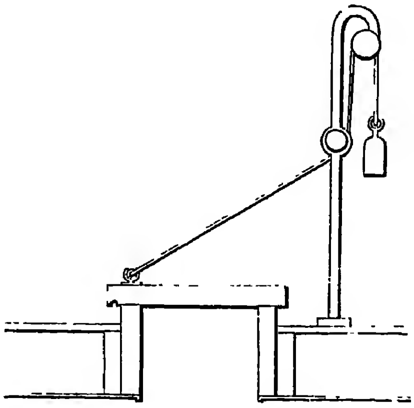


SCALE OF FEET.

Indeed I must confess that there was no sparing of materials in the construction, for, though the transit-room is only eighteen feet by twelve – sixteen feet in height outside and ten feet five inches inside—with an ante-room of eight feet and a half, there were nearly twenty thousand bricks used, reckoning four thousand five hundred to each rod of work. The flooring was laid to a

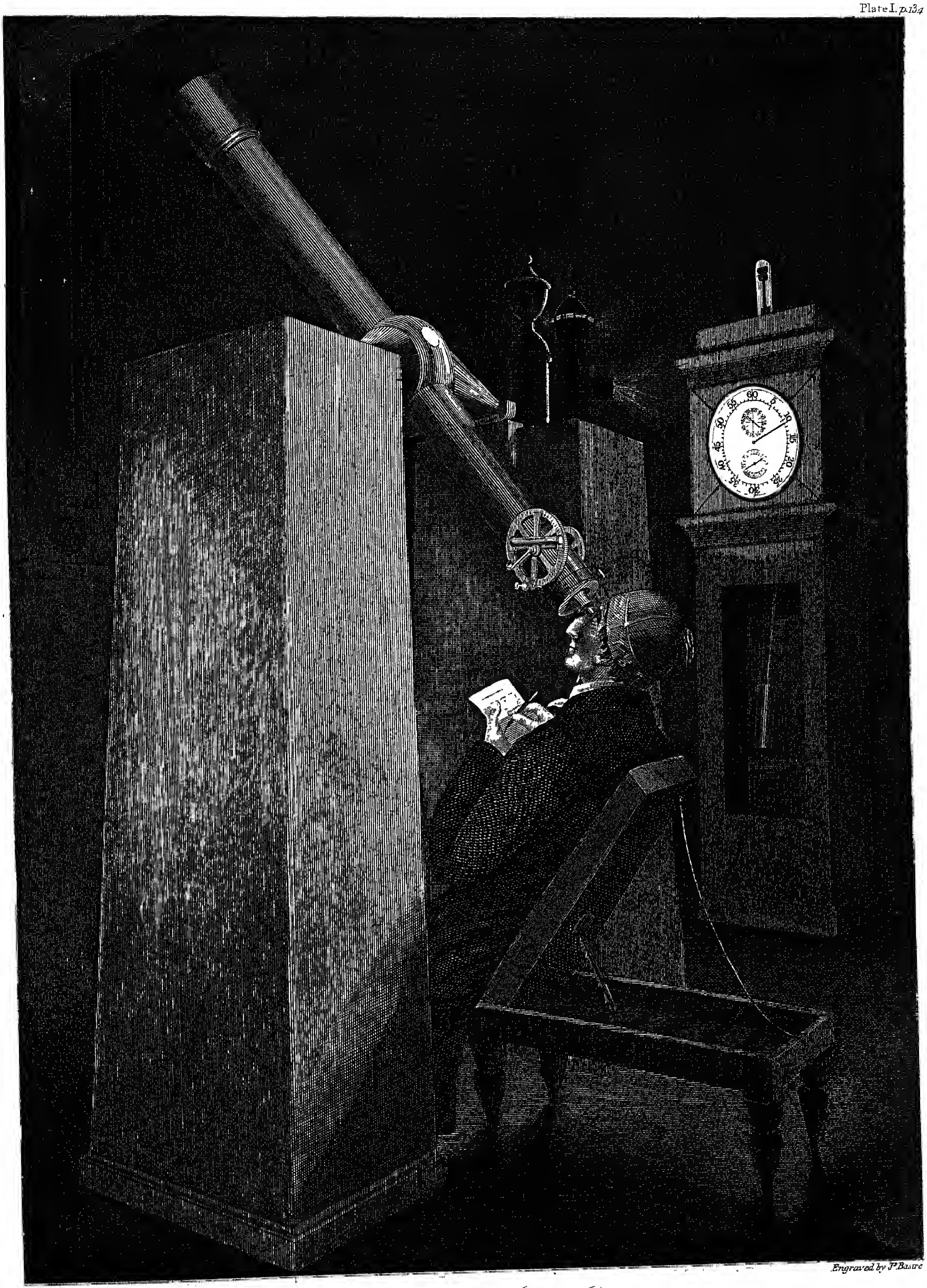
level with that of the inner apartments of the house, and, the wall under the south-east intervening window being cut away, the observatory and library became as it were incorporated, agreeably to the wish and intention already alluded to. The ante-room is fitted for the keeping and trimming of lamps, by which the chance of dirt and oil among the necessary books and papers is lessened; and a flight of steps leads to its flat and well-leaded roof, on which the five-foot achromatic telescope was occasionally placed for use.

From the form of the eastern portion of its wall, and the interior lockers fitted on the west, the transit-room is octangular in shape, with its centre cut by the north and south windows, and the *chax* or slit through the roof from the north to the south horizontal points. I had made arrangements for the shutter which closes this being counterpoised with weights, as in my own at Bedford; but I one day found that a busy-body had suggested lifting it by a crane-necked contrivance which I never could approve of, although good John May, formerly an armourer in the navy, but now lying in Hartwell churchyard, had made it of most unexceptionable materials and workmanship. I submit a representation of it as a warning to meddlers, because what is termed at sea an "Irish purchase" is shown by the position of the central pulley; had it been a little lower, the cosine of the chord between it and the shutter would nearly have assumed the maximum state of helplessness. Even now, its chief action is that of battering the roof and shaking the ceiling—



During the time this transit room was being constructed, Mr. Thomas Jones, of Charing Cross, was employed in getting the transit-instrument ready, under my immediate inspection; and, as simplicity with capability for duty forms a very considerable point with me in all mechanism, and the workmanship was limited to only what was necessary, it was soon completed. The telescope has an aperture of three inches and three quarters, and is sixty inches in focal length; it carries two reading circles divided on silver and furnished with clamps and tangent-screws; and it is efficiently provided with a full battery of eye-pieces, sliding plates for them, and rack-work motion to moderate the lighting-up of the lines within the telescope. It is borne at the centre by two well-proportioned cones, having a circumference of twenty-five inches at the telescope, which terminate in rigorously turned pivots of bell-metal of one inch and a quarter in diameter: one of these is perforated and fitted with a lens, to communicate the light from a lamp upon the pier to the inner illuminating reflector, and from thence to the spider-lines, or wires, at the eye-end. These pivots, which are thirty inches apart, rest upon Y's, also made of bell-metal; but, whether wisely or not may be questioned, the latter are additionally hardened by the introduction of a thin slice of Brazil pebble. The bearing of these applies to the whole length of the pivot-cylinder, whence the counterpoising is so regular that the instrument is pointedly steady at every altitude in the plane of the meridian. A well-constructed sensitive riding-level is placed over the cones when required, and stands upon both pivots; its glass tube is supported by the middle, and not by the ends; and it is furnished with a cross level and screw-adjustments, with counterpoise weight, for levelling it at right angles to the main axis. The whole is exactly on the plan of that which I had made for myself, and mounted at Bedford, as described in my Celestial Cycle, vol. I. p. 329, from which it only differs in focal length, and a little in aperture (*see the plate*). It was completed at a cost of 105*l.*

Meanwhile Mr. B. L. Vulliamy, of Pall Mall, had undertaken to make a clock for the transit-room, and a better train of wheel-work cannot be turned out of hand; but the beat is certainly inferior to that of Hardy's escapement.



Engraved by J. Basire

The Hartwell Transit Room

The frame is a particularly strong one, and all the parts are screwed together. The wheels and pinions are cut in very high numbers, which renders the action of the wheel-teeth in the pinions extremely smooth; and the combination is such, that it seems to have all possible means to overcome its own friction. The escapement is Graham's dead-beat, with steel pallets mounted in a brass frame: and it will be recollected that the preference for this form arises from the dead-beat being so true, that no variation in the clock-train is likely to have effect upon the time of oscillation of the pendulum, even though it may alter the extent of it on the arc of vibration. The pallets are portions of a ring, and the fittings of the arms that carry them are entirely formed by turning. There is an adjustment to the pallets to open and close them, so that the teeth of the wheel shall fall safe on the face of the pallet, and no more. The pivot-holes are all made of fine pan brass, mounted in a setting, and the end-shake determined by regulating-screws. The pendulum is suspended upon an independent support, entirely detached from the clock, the sole connexion between which and the pendulum is through the medium of the crutch, and consists of two adjusting screws. The clock was brought to Hartwell, and singularly well fixed, on the 14th of November, 1832. By the side of its solid mahogany case two upright carriages and plates arise, in order to support two large brass brackets and stands, on which the lamps are placed for illuminating the clock's face; above which stands a Hardy's "noddy." Its price was one hundred guineas, besides incidental expenses.

As I had used a pendulum contrived, on my own suggestion, by Mr. Jones of Charing Cross, and considered that it obviated certain faults in the usual stirrup support of a mercurial cylinder, I wished that Dr. Lee's clock should be also fitted with one. Mr. Vulliamy's assent to this proposal was thus expressed to me, in a letter dated June 27th, 1831:—

I am much flattered by the confidence Dr. Lee reposes in me, and will spare no pains to make such a clock as shall give him perfect satisfaction: at least as far as is in my power.

With regard to the pendulum, after what you have said, I will with much pleasure make it as you require; but I must claim from you the promise which you made me when I saw your clock

at Bedford, of obtaining from Mr. Jones the mode of applying the brass case or covering to the steel rod.

You must excuse my making a positive promise as to when the clock shall be done. I depend a good deal upon others as well as upon myself, and with the best intentions I frequently am lamentably deceived as to the time in which I can execute a piece of work. In this case I feel I am under an obligation to you, as well as to Dr. Lee, to get the clock done as soon as practicable, consistent with its being well done, and you may rely upon my so doing.

What chance is there of my father's fine clock being admitted into your Observatory? I have an idea it would perform uncommonly well.

The pendulum thus alluded to, is still in high favour with me after more than thirty years' acquaintance with it, and is therefore entitled to especial mention. This very simple and effective adaptation consists of a round steel rod, long enough to pass through the mercury, and nearly to the bottom of the inside of the cylinder containing it. The suspension-piece at the top of the rod is a bit of watch-spring, in the usual way: but the lower end of the rod has a screw on it about two inches long, the threads of which are each equal to one-thirtieth of an inch. This screw works into an appropriate piece fixed to the metal bottom of the cylinder's interior, thereby allowing sufficient adjustment, according as we would compensate for mean or sidereal time: and the inside of this recipient is carefully and well coated with cement and gum lac, so that the quicksilver can never come in contact with the brass surface. The steel rod is covered loosely with a thin brass tube, which is pinned to its upper end, and passes below to the top of the cylinder, over which is a brass cover with a slit at its side, embracing a pin which projects from the cylinder's outer surface. The broad edge of the cap is engraved and divided into one hundred and twenty parts; and, as the pendulum-screw has thirty threads in the inch, it is evident that the unit of the scale by which the cylinder with its mercury may be raised or lowered, is the $\frac{1}{3600}$ of an inch. A steel point for marking the arc of vibration is fixed on the bottom of the cylinder; and there are screws for the necessary adjustments.

As the excellence of the mercurial pendulum depends on the relative action of the steel rod and the quicksilver, it is evident in this instrument

that they are in constant communication with each other, for full six inches of the rod is always immersed in the mercury, which is not the case in the usual construction, where they are never in contact. Moreover, both the rod and the mercury are covered with the same material, which gives them a fair chance of acting together; whereas in the stirrup-construction the quick-silver is contained in a glass vessel—a bad conductor of temperature, while the rod is naked and exposed to the air. Hence the rod may undergo many changes of heat and cold that the mercury is defended from; and the additional hamper which the old construction requires, must occasion a greater drag, however difficult it may be to appreciate the amount of such an effect.

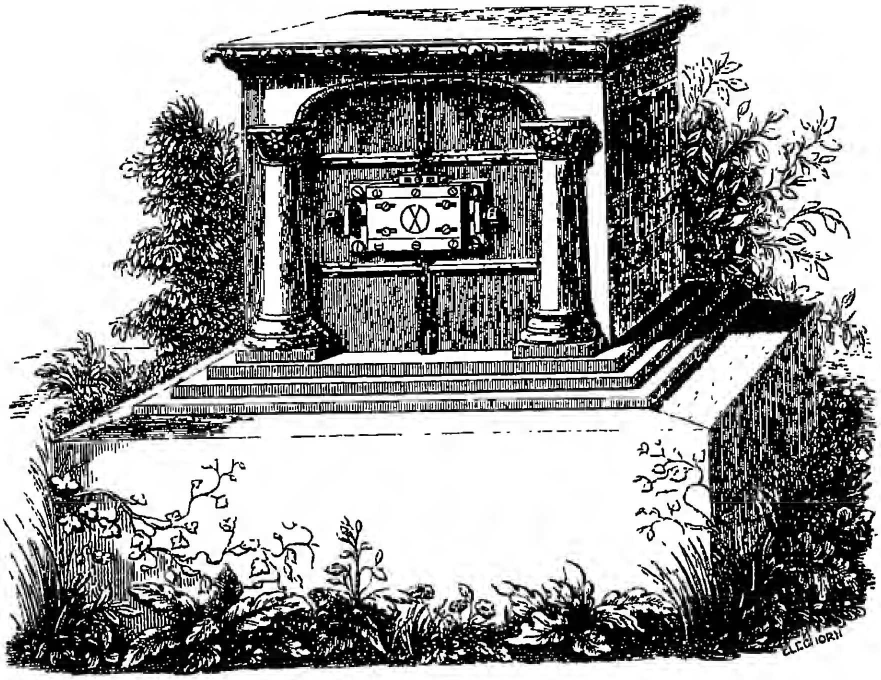
Another point in which I have been rather nice in a clock's performance is the weight, which is often sufficiently rude to wear the pallets and endanger the pendulum spring. I therefore directed one to be made in divers cylindrical pieces, like my own, so as to be capable of being adjusted to any quantity between four pounds and nine pounds, within one quarter of an ounce. If a pendulum vibrates two degrees on each side of the zero point of the arc, that marks as much weight probably as ought to be employed permanently: at the same time there is no objection to enlarging the arc of vibration, by increasing the maintaining power to a certain extent. In the Hartwell clock I found that, with seven pounds one ounce, the pendulum vibrated one degree fifty-four minutes on each side of 0, a range to which it has been confined.*

Much of this will be sufficiently trite to the magnates of practical science; but that is not altogether the class to whom these remarks are addressed: and fortunately it was never intended that the world should be peopled only with giants. Leaving the leaders, therefore, to their experience, knowledge, and plenitude of power in the great observatories; we are here only bent upon showing any tyro, how he may enjoy the glorious heavens without bewildering himself in severe mental and mechanical drudgery, and meet the smiles of

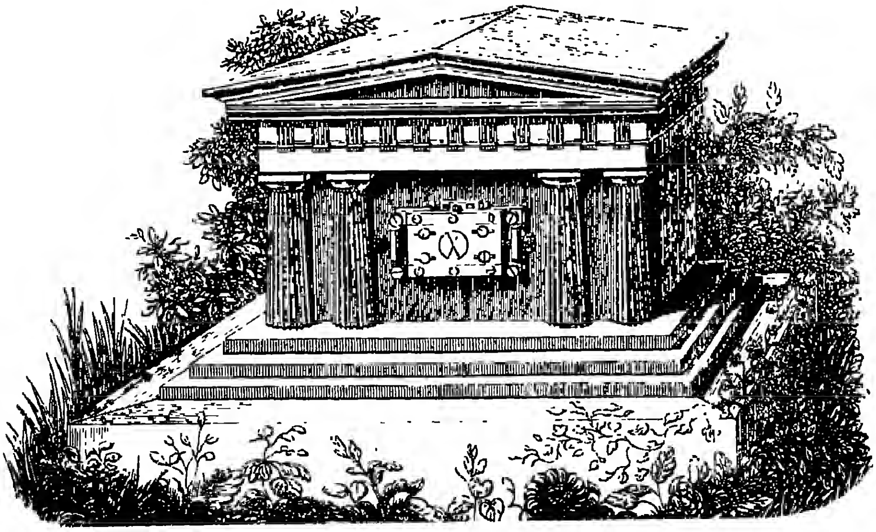
* It may be as well to advise those who meddle with the weights, to stop the clock on such occasions; as the pendulum should not on any account be suffered to vibrate when not under the influence of the maintaining power.

Urania without apprehension of encountering her frowns. It is therefore quite in place to insist on the advantages which a private establishment of this nature obtains, from deducing a due registry of time, and to point out that the keeping an accurate clock-rate is one of the neatest works of a prepense observer. The machines are, to be sure, so admirably constructed now-a-days, that little irregularity in the march need be dreaded in an interval of two or three days. But such are the imperceptible affections of escapement and pendulum, train and its drivers, in impulse and momentum, that no really zealous star-gazer ought to leave their errors so long unascertained. Where occasional absence or bad weather interpose and prevent a due examination, then the goodness of the clock must be relied upon; and it is on this account that every practical man should be provided with mechanism capable of such good performance, as to aim at excellence. Constructive ingenuity has certainly advanced so near to perfection, humanly speaking, in this art, that a clock of any claim merely requires to be well treated and looked to, and it will render the most satisfactory results.

The clock being fixed, and the transit instrument rigorously drawn into the meridian by reference to the pole and circum-polar stars, it became necessary to establish permanent points of reference, by which not only the meridian, but other adjustments also, might be tested from time to time; and, as distant objects cannot always be seen in the day, and are precluded at night, I determined upon strengthening Dr. Lee's means by two of the meridian marks, of which I had found one so useful at Bedford (See Astronomical Society's Memoirs, vol. iv. p. 548). To mount these with a propriety due to the site, we had them inserted into blocks of marble cut at Bedford, from drawings made by Mrs. Smyth. The north mark is a representation of the Temple of Janus, as given on a large-brass medal of Nero, inscribed PACE POPULO ROMANO TERRA MARIQUE PARTA JANUM CLUSIT. See my Descriptive Catalogue of a Cabinet of Roman Imperial Medals, printed at Bedford in 1834, for private circulation, page 43; for the coin there described, Number 50, is that from which the temple is copied:



This meridian appliance was suggested to me by Baron de Zach, on our journey from Genoa to Bologna, to catch the solar annular eclipse of September 1820. The south mark is exactly the same as the north one,—but it is mounted on a miniature of the façade of the Temple of Concord at Girgenti, with its central columns omitted for the insertion of the meridian plate :—



In order to suffer as little alteration as possible from corpuscular or other action, these marble temples were placed on stout basement stones upon a solid brick-work foundation; which last was, for the same reason, carried no higher than was absolutely necessary. They were placed respectively at the distance of one hundred feet north and south of the observatory slit; and the reference was by means of two lenses, one in each window-sill, ground to exactly 100-feet focus, and mounted in brass frames with tubes through the walls. The marks could of course be readily shown whenever it might become necessary, at any hour of the night, by means of a small hand-lamp and a simple tin reflector on a stand.

It happened that, while these arrangements were in hand, Mr. Davies Gilbert, late President of the Royal Society, came on a visit to Bedford, and was persuaded to accompany me to Hartwell, and inspect the new observatory. On being shown the transit instrument, and acquainted with the end proposed, he warmly approved of the intention: "for," said he, "it may be very unexpectedly useful, as stars may be taken here when the sky shall be obscured at the regular observatories. And how curious it will be if a place in Kamschatka, or China, or other distant region, should have its longitude determined some day or other from Hartwell!" Since this remark was uttered, Dr. Lee's records have been ransacked on several occasions, for he is disposed to attend to every proper application which is made to him. But perhaps the nearest fulfilment of the Gilbert prediction was, when the late worthy and hard-working Admiral Philip Parker King, R.N. of New South Wales, who was a visitor at Hartwell in August 1849, found eight moon-culminating stars—corresponding to those he had observed in Australia in the years 1845, 6, and 7,—which he had in vain endeavoured to obtain elsewhere

In this recollection, I am not insisting on the merits of moon-culminating stars beyond values of a certain degree. They are by no means adapted for determinating small differences of longitude with precision; for which nothing under one hundred complete sets, namely, fifty of each limb of the moon, should be attempted. With a fixed transit instrument the Greenwich

clock-stars may be used as culminators, though they are not, as with the selected stars, equally above and below the moon; and, in all cases of small arcs, a single journey with a batch of chronometers will yield the longitude better than one hundred transits of the moon, which would occupy perhaps a couple of years, besides being attended with great trouble of computation. The longitude from one lunar culmination is not unfrequently twenty-five seconds of time wrong. But, with all the possible imperfections on their head, my reason for recommending the consecutive observations of them was on account of there having been, about that time, a kind of conventional adoption of them in the observatories of Europe.

Although Mr. Maclear—the present distinguished Astronomer-Royal of the Cape of Good Hope—and myself got a good arc between Biggleswade and Bedford by their means, as shown in the Memoirs of the Royal Astronomical Society, vol. iv. p. 564, still, on several computations, I found that the results for such small differences would inevitably be anomalous. But this does not apply to larger arcs: there, while other methods might err greatly, or be seldom available, the moon-culminators are not affected with greater errors and uncertainties than in the small distances. Yet, in all cases demanding implicit faith, a large number of well-taken observations of both limbs must be accumulated for discussion.

Before quitting this meridian-room it may be as well to make allusion to the new mode of taking the transits of culminating bodies, so meritoriously adopted by our trans-Atlantic brethren, but so hastily therefore designated the "American method:" for, if by that name it is to be considered an American invention, a reclamation must be made. In doing this I will not proceed by citing the crude hints of fancy and vague suggestions, or by conjuring up allusions in unproduceable letters from A to B: my averment shall rest upon public exhibitions and public experiments publicly reported in the official records of public societies; and which were published and circulated throughout the world, several years before the said method was used on the other side of the Atlantic.

Among the various applications which the skilful Professor Wheatstone * has made of the principles of his electro-magnetic telegraph for different scientific and industrial purposes, he described to me the means by which he enabled the time of a single clock to be shown simultaneously in a great number of places, or in other words to telegraph time instead of talk. He then fully explained that which I afterwards saw in action, namely, the wheel for making and breaking the circuit, and the action on it by the arbor of a clock, which forms the main feature of reciprocating motions. This chronoscope, or, as the Professor then called it, telegraph clock, was described in the *Bulletin de l'Academie Royal de Bruxelles* for October, 1840, by M. Adol. Quételet, the able Director of the Belgic Observatory. From that description I will here submit an unanswerable passage—

Sous le point de vue scientifique, les résultats qu'on peut recueillir des télégraphes electriques de M. Wheatstone sont immenses. Ainsi, pour les localités par où passera la ligne télégraphique, *la détermination des longitudes*, l'une des opérations les plus delicates de l'astronomie pratique, n'offrira plus la moindre difficulté. D'une autre part, d'aprés une disposition particulière, une pendule peut donner l'heure à toute une maison, à toute une ville, même à tout un pays: les pendules auxiliaires qui marquent les heures, les minutes, les secondes aux même instants que la pendule regulatrice, ne se composent que d'un simple cadran: aussi M. Wheatstone les nommé squelettes des pendules. L'auteur compte aussi employer ses procédés pour mesurer, avec une précision qu'il croit pouvoir porter à un centième de seconde, la vitesse des projectiles. Il serait difficile de limiter les applications auxquelles se prêteront les ingénieux appareils du M. Wheatstone. Néanmoins l'un des plus beau titres scientifiques de l'auteur, sera toujours d'avoir mesuré l'incroyable vitesse du fluide électrique qu'il devait employer si heureusement plus tard.

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* This gentleman, the most remarkable inventor of our day, has had much obfuscation to put up with. Though he is undoubtedly the first contriver of the Electric Telegraph in the form which made it available for popular use, yet shaky claims were *Cooked* up in other quarters. His discovery of the elegant Stereoscope was carped and nibbled-at, and his sub-marine Telegraph, of which he showed me plans, and publicly explained the details, upwards of eighteen years ago, has been all but smothered in other names; hence the Abbé Moigno, in his *Traité de Télegraphie Electrique*, pointedly says, "*M. Quetelet avait annoncé, dès* 1840, *que M. Wheatstone avait trouvé le moyen de transmettre les signaux entre l'Angleterre et la France, malgré l'obstacle de la mer.*" I am not aware of any cuckoo yet sitting upon his incomparably simple Polar Clock, though invasion may be looked for. *Ma spetta un poco!* Time is a great adjuster of these anomalies!

Two papers by Mr. Wheatstone on electrical subjects ought to be recommended to the reader, namely, "Experiments to determine the velocity of Electricity, and the duration of electric light (1834); " and "New instruments and processes for determining the constants of a Voltaic Circuit (1843)."

Inventions which are based on experiment cannot—like Minerva—start into the world armed at all points; it is therefore not in my power to state how long the above-mentioned matters may have been in hand before they were thus communicated. Enough, however, remains for the ends of justice, for I had frequently the pleasure of discussing these topics with Wheatstone, he ever being liberally communicative. In November, 1840, I was present when his clocks were shown in full action and described, not to an unciteable individual, but to a large meeting of Fellows and Visitors of the Royal Society, under all the scope the various rooms of their apartments would admit of. The description then given was published at the time in the authorised "Proceedings" of that body, and the inventor's view is thus expressed—

The object of the apparatus forming the subject of this communication is stated by the author to be that of enabling a single clock to indicate exactly the same time in as many different places, distant from each other, as may be required. Thus, in an astronomical observatory, every room may be furnished with an instrument simple in its construction, and therefore little liable to derangement, and of trifling cost, which shall indicate the time, and beat dead seconds audibly, with the same precision as the standard astronomical clock with which it is connected, thus obviating the necessity of having several clocks, and diminishing the trouble of winding up and regulating them separately.

At or about this time he also recommended some further appliances for astronomical purposes, by which the chronoscope became a chronograph; and, to my personal knowledge, he strongly urged to some leading astronomers its importance for registering transit-observations. Further notices of his invention appeared in the Bulletin of the Brussells Academy for May 1843, the Electrical Magazine for October 1844, and the Comptes Rendus of the Academy of Sciences at Paris for May 26th, 1845. Now the first attempt to determine a longitude in America by means of the electric telegraph, namely, that between Washington and Baltimore, was conducted by Captain Wilkes in June 1844, four years after Wheatstone had been endeavouring to incite European *savans* to the step, and after the practicability of it was published to the world. This is not exactly the arena for a lengthy discussion of the subject; but, with great regard for my friends the Bonds, and all due respect

for the Lockes and Mitchells in their laudable exertions as to noting the passages of celestial objects over the meridian of a place by the agency of a galvanic current, I feel compelled to reduce their noble to ninepence. Armed with a perfect recollection of facts, and supported by indubitable documents, I therefore lodge a hearty protest in favour of Wheatstone's priority of invention; and I ratify my declaration with this *resumé* of the argument:—

It is clear from various official published records that, so early as 1840, long before the Americans had moved in the matter, Professor Wheatstone had seriously proposed the application of the electric telegraph to determine terrestrial longitudes, had openly mounted the electro-magnetic clock by which the apparent time at different stations might be accurately compared, and had constructed an electro-magnetic chronoscope, by which the most minute differences of time might be determined.

These inventions were first announced in the Bulletin of the Academy of Brussells for October, 1840. The description of the electro-magnetic clock was published in the Proceedings of the Royal Society for November the 26th, 1840, and that of different electro-magnetic chronoscopes in the Comptes Rendus of the French Academy of Sciences for May 16, 1845. The latter also contains the description of an instrument similar to those since used by the Americans in their transit observations after 1849.*

Liberavi animum meum! but a word more upon the subject of catching a transit under untoward circumstances. On visiting Oxford some time ago, I found that Mr. Johnson had fitted a simple apparatus to his transit-clock, in order to insure the hearing of its beat in windy weather, or under accidental noises; and, on making a trial, I was satisfied of its utility in certain cases. Mr. R. C. Carrington, acting by Johnson's advice, had also procured and used one; and, in answer to an inquiry which I made, replied, "I found it of the

* I am but indifferent authority on the point, yet I must confess, observatorially speaking, that at present I see little more in the chronograph than a most ingenious piece of mechanism, for, as far as I yet know, I could have obtained as good a transit with the usual instrument in my palmy days of seeing and hearing at a far less cost of time and money. Though the chronograph may cure personal equation and show a probable error, such errors are not common in other registers. A tolerable night's observations will require a couple of hours unravelling and reading off, to render graphical expressions into figures, a part of the practice, however, which is still susceptible of improvement. Moreover it is still to be shown that the nerves of the eye and the ear are not in as immediate a connection as those between the eye and the finger.

As to the synchronistic alliance with other observatories, though it is an elegant and wonderful convenience to have at one's command, I cannot predict that the luxury will be followed by any signal astronomical advantage. The application of electricity to Time Balls at our sea-ports is, indeed, beneficial.

greatest use at Durham in high wind, and whenever there was a general noise of any kind." Wishing therefore to know something more about it, I addressed to the Radcliffe Observer these queries, namely: 1. Are you the inventor of what I shall now call the *Hearing Tube?* 2. Of what material is it made, and how? 3. Is it easily procurable and reasonable in price? 4. How do you apply it when observing? 5. How have you found it answer in practice? To these questions he answered, June 19th, 1858, as follows:—

In reply to your curt and pithy inquiry about the *Hearing Tube*—

1. I know of no preceding application specially to a transit-clock; but I take it that the inventor of the ordinary hearing apparatus may partly claim the merit of this also; and he is quite welcome.

2. It is of the same material as the ordinary hearing tube—vulcanized India-rubber—with an iron spring running through; the spring, in our case, might, I think, be advantageously dispensed with. Carrington's is a length of gutta-percha tubing, of about half-inch bore.

3. The cost is trifling, and with the alteration suggested above it would be still cheaper. But the saucers, of which I shall next speak, were made of ivory for Carrington.

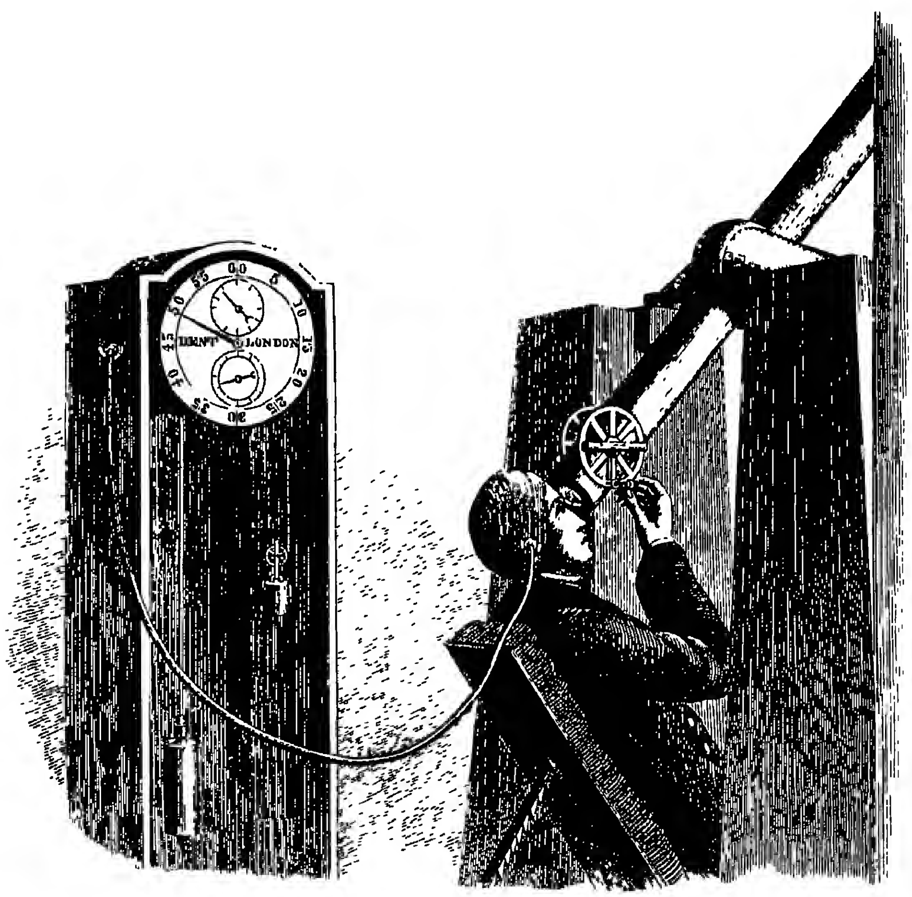
4. At the ends of my tube two, nearly flat, saucers of wood are attached, having a hole in their centres for the passage of sound. One of these saucers slips into a frame or socket screwed upon the clock-case. The other end the observer holds to his ear. We found it was unnecessary to perforate the clock-case.

5. I have often saved an observation by means of it when the clock beat would have been inaudible. It requires a little practice, since the disturbing noise of course will come into both ears of the observer, and the tube is applied only to one. I had a small hole cut in my observing-cap, with a little frame made of card-board, covered with silk, sewn on the outside of the cap. Into this frame I used to place the saucer at the end of the tube, and slip the cap on my head till the orifice of the saucer came opposite my ear. In doing this I also, of course, covered my other ear; which served in some degree to drown the sound of the wind, or other disturbance I wished to avoid; but to do so more completely, I used to put my hand to my disengaged ear when in the act of observing.

This certainly was not so agreeable as if you had no appendages about you; but it was a case of being able to do *anything* or *nothing*. I never was beaten by any other noise than that of a train passing on the railway at 600 yards off. This is my own experience, and I won't answer for any body else. I may add, that it was necessary sometimes to press the saucer to my ear closer than it would have been if left attached only to the cap. In this case I used, when looking towards the zenith, to lay my note-book on my chest. If observing towards the horizon, the book may be easily held in the hand at the same time that you are pressing the end of the tube to your ear.

It is to be hoped with the lamented Johnson's lucid explanation, and the

subjoined illustrative sketch by Mrs. Smyth, that the amateur who is desirous of applying the Hearing Tube will readily accomplish it: and the importance of affording every aid to the operation of securing a good passage over the wires will become evident on reading the able essay of Dr. T. R. Robinson, of Armagh, on the "Probable Errors of the Eye and Ear in Transit Observations." During the passage of a celestial body, the ear counts the clock beats, while the eye watches and notes the distances of the star from the wire at the beats which precede and follow its orderly march across the field: "and thus," he remarks, "the mind estimates the fraction of the seconds from the relative position of three points; all existing in memory alone, when that estimation is made."



CHAPTER IV.

THE EQUATOREAL TOWER AND TELESCOPE.

"What crowd is this? what have we here! we must not pass it by:
A TELESCOPE upon its frame, and pointed to the sky."*

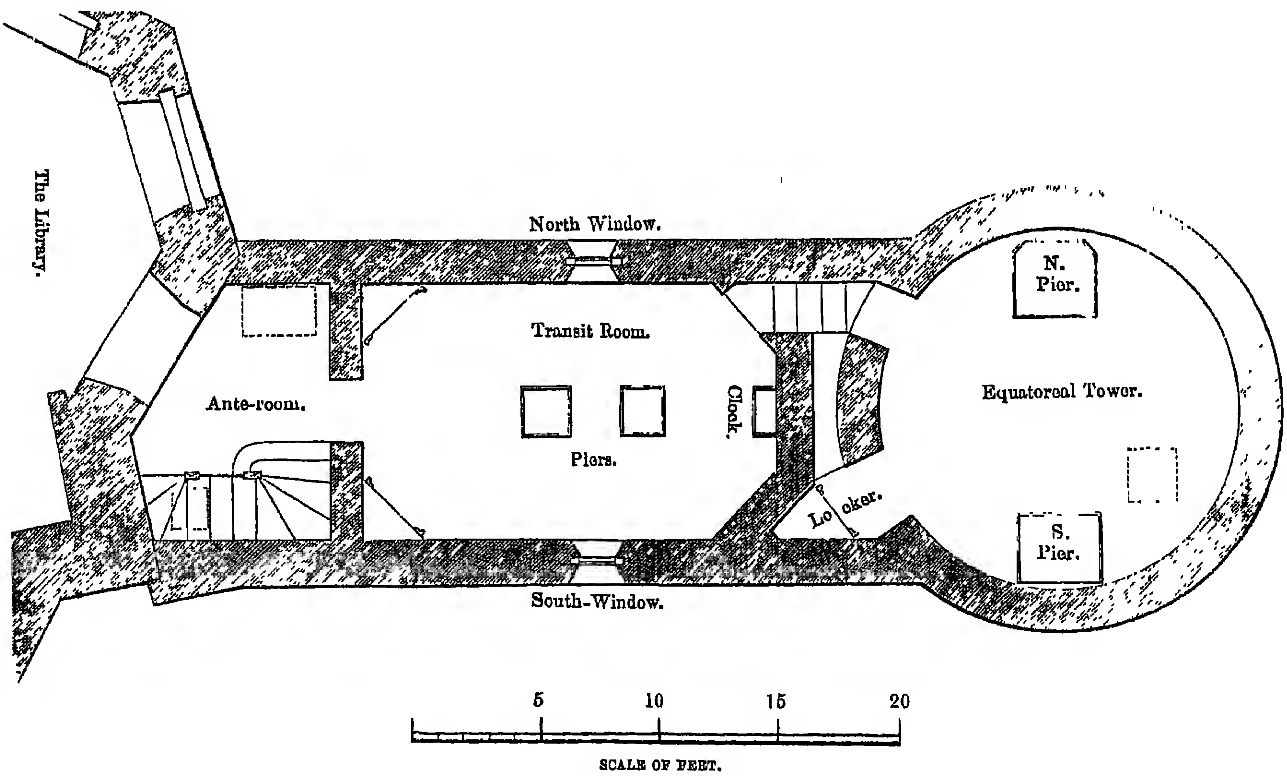
WORDSWORTH.

NOTWITHSTANDING that contentment is a virtue much vaunted in morals, it has not acquired great esteem in the sciences: nor was it more than three years after building his transit-room, and furnishing himself with the beautiful five-foot telescope, that Dr. Lee compassed the enlargement of his astronomical means, by purchasing a splendid object-glass, of five inches and eight-tenths in diameter. On his making this acquisition, I was again consulted as to adding an equatoreal tower to the meridian observatory. I had, to be sure, sundry scruples on the occasion, inasmuch as the site, though very excellent for a meridian instrument, was extremely inapplicable for one which would be expected to sweep around: and I moreover maintained that the finely-figured five-foot achromatic with which I had supplied him was fully equal to

* The poet tells us that this telescope was placed in "Leicester's busy square"—a square which I passed one fair night with some A 1 Astronomers, on our return from an evening meeting. Here we found a scene exactly as that described in the "Star-Gazers;" and, on making inquiry, we find the "Showman's" charges were—"twopence for the Moon, and fourpence for Jupiter." This is surely reasonable enough to invite all to a peep; yet I have known educated elders not ashamed of avowing, that they had never viewed a planet through a telescope!

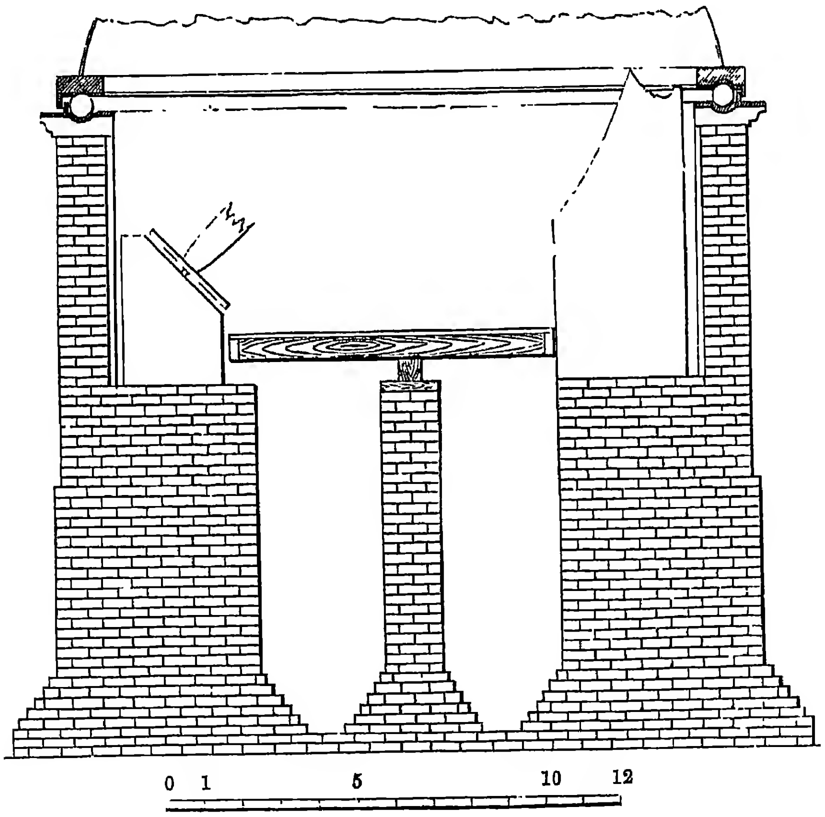
By the way, while alluding to this poem, I am not likely to forget that when, in July 1838, I had the honour of having an honorary degree conferred on me at Oxford, I marched into the theatre between Astronomy and Poetry—Herschel and Wordsworth, with both of whom I had had many years of acquaintance—and we were all "doctored" together; my learned friend the Chevalier Bunsen, of Prussia, being also one of the batch.

occultations, eclipses, and all the extra-meridional requirements of the place. However, after a reasonable time, I withdrew my opposition, and considered the conditions of the problem with attention. In the first place the new object-glass, which had nothing but its brass cell, was carried to Mr. Dollond, with a view to its being armed with eye-pieces, and mounted precisely in the manner of mine at Bedford. We then took steps for fitting the tower symmetrically to the observatory, so as to form, as it were, a part of the original plan; and its erection was attended with the same amount of care and attention to solidity and strength, that had been already adopted in the building of the other portion. The success of this is shewn by the lines of the whole building, as they stand at present:—



When the addition was thus carried in our synod, I called in the aid of my zealous friend Mr. Charles May, then of Ampthill, but afterwards of the well-

known house of Ransome and May, of Ipswich, under whose admirable arrangement the undertaking was finished. Determined that the foundation should be equally stable for its purpose with that of the transit, we again called sifted Roman cement and the very best brick-work into requisition; and the structure quickly assumed an important appearance. Besides the north and south piers for the supports of the polar-axis, a smaller one was run up in the centre, as there was a suggestion for using a smaller instrument there occasionally; and the bottom part of the pier-chamber was well floored with brick laid in cement—resting on a bed of concrete and rubble—by which means the damp liable to arise from the ground is interrupted. And the following is a section through the structure, in a north and south line:—

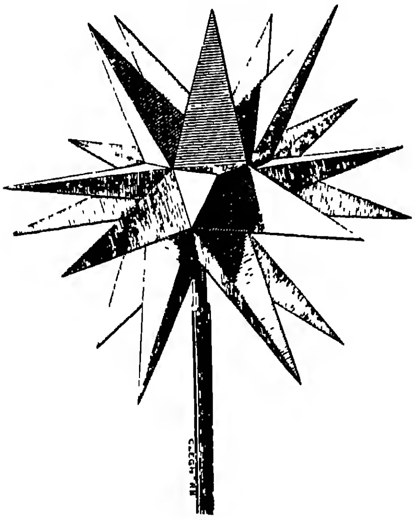


This tower has an interior diameter of fifteen feet, between walls which are fourteen inches thick in brick-work, capped with a Portland-stone coping in six-

teen stout blocks, for bearing the iron channel and curb for the rollers on which the hemispherical dome, rising ten feet higher, turns. By the coping projecting outwards, it forms at once a moulding and, by a groove on the upper side, a channel for the rain to pass off without running down the wall. A cast iron ring, made also in sixteen pieces, rests upon and is partially imbedded in this capping; the joints are made true, so as practically to be like a single casting. In this is a shallow circular groove four inches and a half wide by three-quarters of an inch deep, and of the curvature due to a radius of four inches, in which three iron balls of six inches and a quarter diameter are placed, for the dome to run upon; and it revolves with the greatest smoothness and ease to any quarter of the heavens the observer may wish to examine. The dome, in form, is almost a hemisphere, and is constructed of fir-wood in several thicknesses—each piece about six inches deep by eight inches and a half in width—the inner diameter of the dome being fifteen feet four inches. To the underside of the aforesaid curve is secured a ring of cast iron with a circular channel, being the counterpart of that just mentioned. From the upper side of this curb spring sixteen curved rafters of cast iron, the section of which is that of a ⊤, and the upper ends of these meet against a cast-iron ring of about three feet diameter: fifteen other intermediate rafters of similar section, but shorter, abut at their upper ends against cross-pieces connecting the fifteen main rafters, the sixteenth being left out to form a clear opening for view. The rafters are filled in between with two inches and a half battens, jointed straight, rabbeted in their length, and the ends meeting at the back of the rafters, where they are firmly screwed upon the back ribs of the same. The inside of each piece of wood being flat, the whole interior is, of course, polygonal; but the outside is adzed off to the proper curve, which is covered with thin copper sheathing. The opening for observation is continued to the upper ring, and is equal to one-sixteenth of the circumference: the shutter moves on a pin at the apex of the dome as a centre, and it is carried at the bottom by rollers on a bar fixed to the wood curb; there is also a toothed rack and pinion with a handle inside the dome, to give it motion. The shutter itself is one

piece of hammered copper, less than one-eighth of an inch thick, and it has a ridge on each side for the purpose of stiffening it, and to act as a defence against rain driving in.

About seven years ago, the outward appearance of this dome was greatly enhanced by a characteristic star replacing the former plain spherical knob. The rays of this polygonal star are formed of three-sided pyramids of sheet copper, soldered together, and the whole strongly gilt, in order to stand the weather. The object in having so many rays was, that in almost every position of the Sun, as regarded the star, some of the faces should be visible as reflecting surfaces. The ingenious ornament was made by the late Mr. Benjamin Bate, of London, at a cost of 25*l.*; and after his decease, when his business was broken up, Mr. May obtained it by purchase, and most kindly presented it to the Hartwell Observatory.

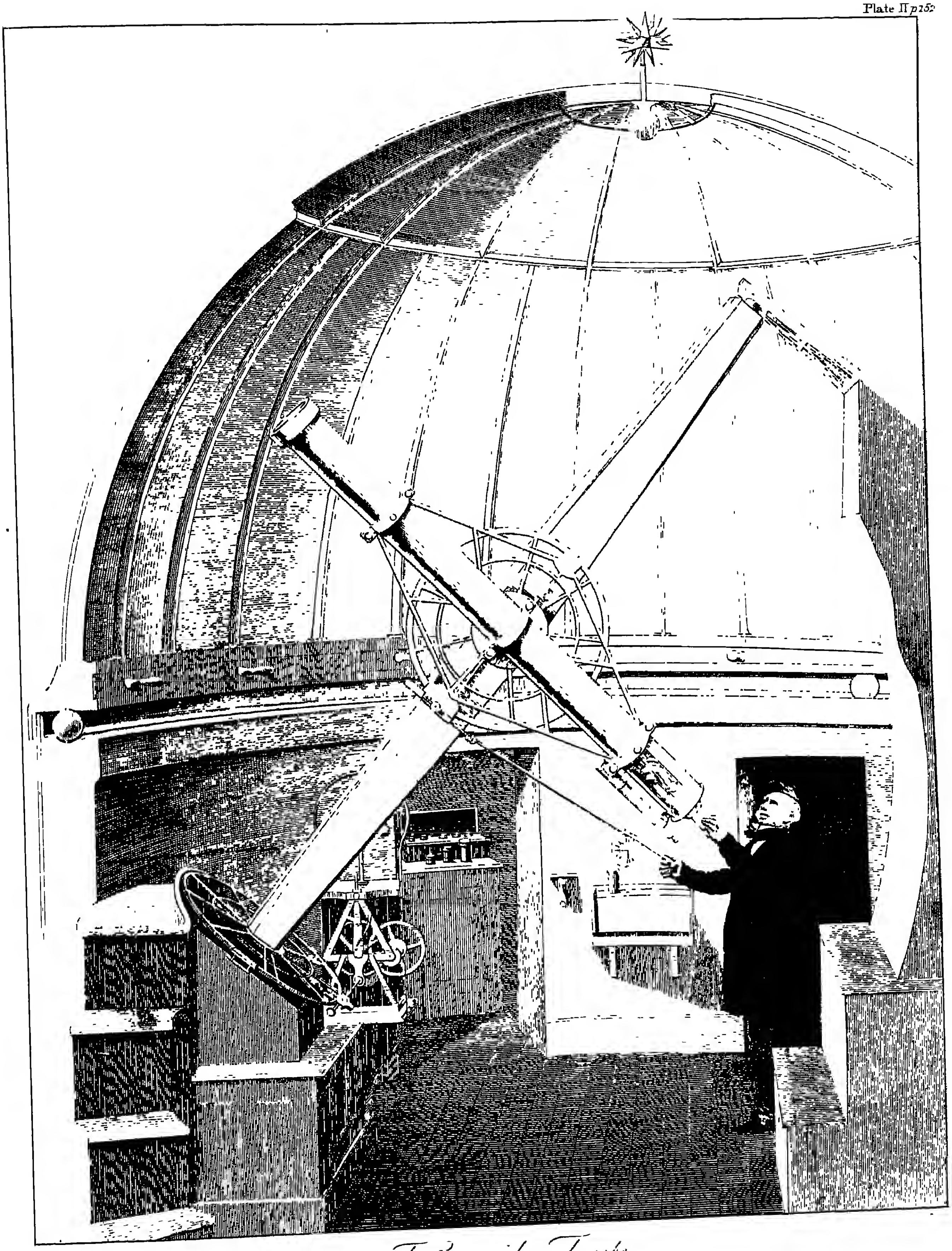


I have been the more particular in describing this roof, not only on account of the great comfort I have experienced under it, but also because the Hartwell Dome may very fairly be termed the first development of that latent talent for observatory engineering in Mr. May which has since been so unequivocally manifested in various undertakings for the Astronomer Royal, Mr. Barclay, himself, and others. When I planned my own revolving-roof at Bedford, economy led me to construct it of a straight-lined conic shape, instead of fashioning the ribs to a semi-circular bearing, as had then generally obtained. But in this for Hartwell, where it was more imperatively necessary to consult appearances than with me, it was determined in a conclave—consisting of Lee, May, and Smyth—that it should be truly hemispherical, should move upon three balls, and should open by a single copper shutter from the zenith to the wall-plate. And the present efficient and durable structure was the successful

consequence, in which the roomy space, the strength, and the revolutionary principles are all equally admirable.

This is proved by the fact, that, though the dome has now been completed about twenty-three years, it has never required repair or alteration, nor has the wet penetrated anywhere. None of the swags between the points of support formed by the three balls, which a weaker construction would have had to endure, have been known: and, from the firmness and accuracy of the wall-plate, these balls were found so little accelerated or retarded respectively that the roof was only lifted, for the first time since it was placed, on the morning of the 21st of September, 1850, there even then requiring but little adjustment, after nearly twelve years of wear and tear—nor has it required regulation since. The mode of raising the roof a few tenths of an inch, just enough to release the balls, and admit of their full play, whenever it is required to restore them to an equidistant position from each other, is by a simple, yet efficient contrivance of Mr. May's. It is by means of two stout little iron windlasses, unattached, and only used when necessary: they each consist of a pair of arched plates resembling the head of a large hammer, with a stout screw, having a moveable capstan-head, turned by a lever, thus affording great power of lifting in a compact form.

But, while the tower was thus in full advance, an unexpected interruption occurred, which temporarily gave the fine equatoreal room to the five-foot achromatic telescope and its tripod stand. This was owing to Mr. Dollond's announcing that, on rigidly testing the large object-glass, he would not recommend it, as quite worthy of so expensive a mounting as had been proposed; and on our going to his house the painful fact was proved. Still, though it did not attain the acme of excellence, it was so far good as to warrant a proper apparatus; I therefore proposed a new and less responsible mode of calling it into action, which was about being adopted, when another circumstance changed its destiny. About the middle of the year 1836, I began to perceive that the object for which I had erected an observatory at Bedford, (namely, a cycle of objects for Amatores) had advanced to within a limited time



The Equatorial Telescope

of its proper accomplishment; and Dr. Lee, who had frequently made use of my great telescope, and was well acquainted with its high qualities, became a candidate for its possession when the materials for my intended astronomical work should be completed. This arrangement was the sooner made, because a future continuance of its use in prosecution of certain sidereal inquiries was most kindly and considerately conceded to me. When, therefore, my round of observations was complete, and other affairs called me into Glamorganshire, my telescope was promoted from its humble location at Bedford to the splendid tower of Hartwell. This very fine instrument has been duly described, both in the Memoirs of the Royal Astronomical Society and in my Cycle of Celestial Objects; but the manner of my becoming possessed of the object-glass is best told in the words of Sir James South, who, in a letter to me from Camden Hill, of the 5th of October 1829, says—

I have brought with me from the continent a piece of flint-glass, upwards of twelve inches diameter, which I am going to have made into an object-glass. I recollect having promised you the refusal of my six-inch one, in case I resolved to part with it; such is now my intention. If therefore you are desirous of having it, pray let me know, as till I hear from you I shall not say any thing about it to anyone else. I do not mean to part with it for less than 220*l.* You will also remember that it has no stand; its clear aperture is 5.9 inches, its tube is of brass unpolished, it has a finder of large dimensions, is provided with illuminator and light-regulating apparatus, and has no eye-pieces. I *believe* it to be Tulley's *chef d'œuvre;* of this however it becomes me to speak with caution, as the instrument is new. Admiral Rossel desires his regards to you and Mrs. Smyth, as does also Baron Zach.

A good six-inch object-glass was in 1829 almost unique; and, as I had had an opportunity of testing the performance of the one in question at Mr. Tulley's, at Islington, I esteemed myself fortunate in securing the prize, although the perfection of my 5-foot made me hesitate. The mounting and equipment were instantly proceeded with; and, though science and practice have successfully laboured to improve equatoreal movements since that time, I have every reason to be still satisfied with its simplicity, stability, and general performance. The principle is that of Mr. Jonathan Sisson's equatoreal-sector, as described by Dr. Vince; in which an artificial polar-axis is placed parallel to that of the earth, with the hour-circle adjusted perpendicular to it, and a true colli-

mation ensured. By thus obtaining a revolving axis in the same direction with the terrestrial one, the attached telescope readily follows any celestial body in its arc of revolution, without the trouble of repeated adjustments for the continual alterations of elevation, which attend all altitude and azimuth methods of mounting.*

Mr. Bishop's large telescope in Regent's Park, and Lord Wrottesley's in Staffordshire, are not only mounted on the same model, but are actually furnished with hour and declination circles cast from the moulds which were cut for me; and they differ so little in other dimensions that the three may almost be termed counterparts of each other, in a conventional point of view. As there is nothing like figures for nailing an assertion, the several sizes of these appropriate amateurs' weapons may be here shown :—

		inches.		ft.	in.
Mr. Bishop's . .	*Aperture* .	7	*Focal length* .	10	2
Lord Wrottesley's	*do.*	7¾	*do.* . . .	10	9
Dr. Lee's . . .	*do.*	5·9	*do.* . . .	8	8½

The hour and declination circles of each, 3 feet in diameter.

Now the first of these instruments, in the hands of such adepts as Dawes and Hind, has fully proved the excellence and facility of its working, in measuring many of the closest double stars in the heavens, in picking up new comets, and in adding eleven planets to our system: and, under the able treatment of Lord Wrottesley, the second has produced a systematic measurement of various double stars, undertaken for the express and difficult purpose of investigating the parallax of those awfully distant bodies.

As this was an amateur experiment of high interest, with double stars, and as the discussion was officially referred to me, I may be excused for

* In usual parlance, this species of instrument has Sisson's name attached to it, because Dr. Maskelyne considered the one made for him by that workman to be an improvement upon that made by Henry Hindley at York, in 1741. But a polar-axis, though without the appendage of graduated circles, was used by Christopher Scheiner—author of Rosa Ursina—so far back as the year 1620.

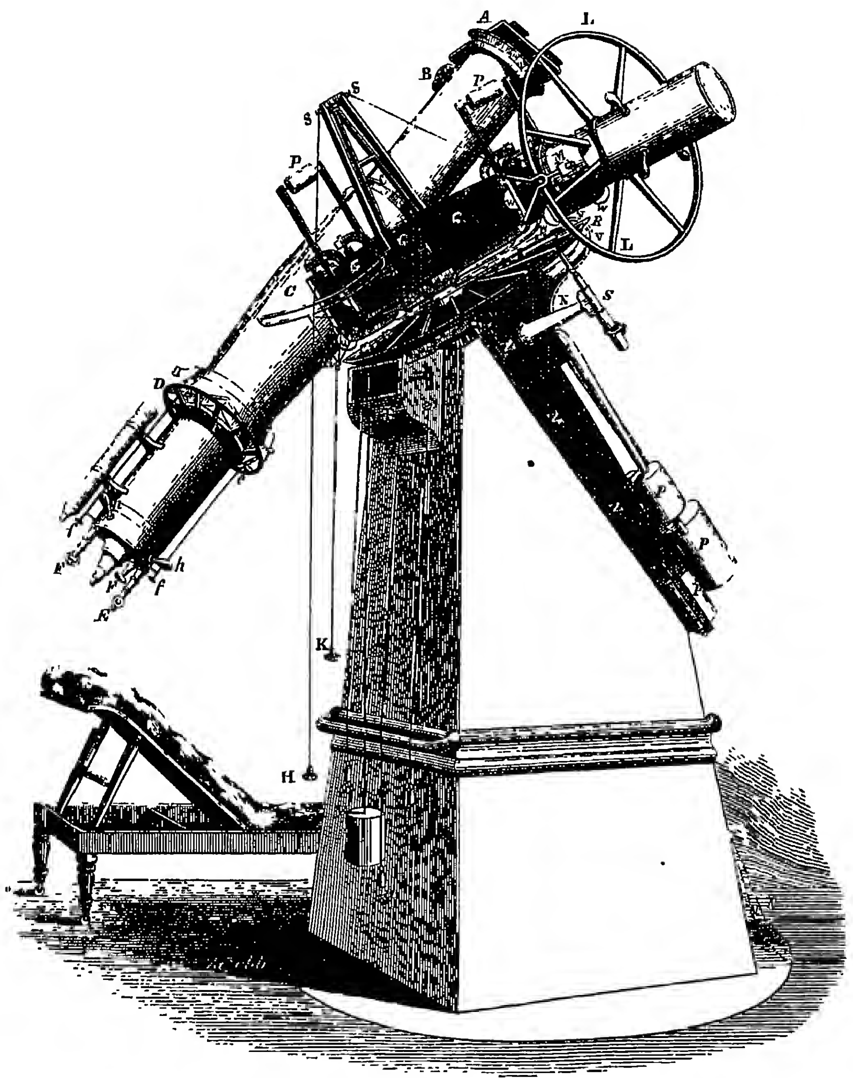
touching upon it. His Lordship's object was to obtain sidereal parallax by periodical observations of the positions and distances of nineteen stars, each carefully selected for the purpose, according to the suggestions of Sir John Herschel, who showed that, if the component members of a double star occupy a certain position with reference to the pole of the ecliptic, and one of them be supposed to be situated within a given distance from the Earth, a change will periodically be produced in their relative positions, consequent on the motion of the Earth in her orbit. The attempt to ascertain this by a direct and continued series of close operations, was highly honourable to his Lordship's zeal and talent. For the stars selected, and the whole *modus operandi,* I must refer the inquiring astrometer to the Philosophical Transactions for 1851; but, for the general reader, it may suffice to hear his Lordship's peroration:

> Omitting, therefore, the five stars whose components are of equal magnitude, omitting also the four binary stars, and two Comæ Berenicis, and ε Draconis (which exhibit differences less than their probable errors), this interesting result appears—that, of the eight that remain, there is only one, that is, 41 Aurigæ, the changes in whose angles, however small and little entitled to confidence they may be, do not conform in direction to those which would take place, were a sensible parallax admitted in the brighter of the stars themselves.
>
> This is most probably only an accidental coincidence, and I am very far from wishing to estimate it at more than its real worth; but, in the case of 32 Eridani, and 1 of H 95, where large, and to a certain extent trustworthy, differences concur with normal directions of motion, it may not perhaps be too much to assert, that this constitutes them objects of interest to the astronomer possessed of adequate means to prosecute an inquiry which, I fear, I must be said rather to have attempted than to have succeeded in.

For the benefit of forming an accurate opinion upon equatoreals, I have examined the telescopes of various observatories; and especially some of those which are termed German mountings, such as those of Mr. Dawes, Mr. Barclay, and Mr. Peters. Though I class many of these discussions among the *difficiles nugæ* of those who hunt in a circle, and hold him to be a poor observer who cannot make the best of whichever of the two he may be provided with—on the wholesome axiom *bisogna adattarsi*—I certainly prefer the POLAR-AXIS: and for these reasons—it admits of the diameters of hour and declination circles being made large enough to brace the telescope, and ensure firmness of clamp

with smoothness of worm-action. Besides which, it supports at both ends the mechanism in motion, to the lessening of variable stress and diminishing tremors of every kind. But the strictest comparison which I had, was that at Oxford. It was on the occasion of my closing accounts, as I then thought, with γ Virginis, after attending pretty closely to her for twenty years. Having completed my measures at Hartwell on the 17th of April, 1850, I immediately started off for the Radcliffe Observatory, and arrived in time to get some observations of the same star on the evening of the 18th, with the grand heliometer by the brothers Repsold of Hamburgh, which had just been got into good action by my energetic friend Mr. M. J. Johnson. This was a severe trial of the powers of the Hartwell telescope; for the heliometer is furnished with the best object-glass that Germany could furnish, and all the appliances attest the assiduity and care of Mertz and Sons. It certainly is a splendid instrument, with excellent optical definition, and is, no doubt, destined to render most important services to physical inquiry. Its object-glass is 7·5 inches in diameter, with a focal length of ten feet four inches: the scale is read by the illumination of a beautifully contrived application of Grove's galvanic heat upon a platinum wire; the appliances for manipulation are most conveniently arranged, and the clock-work driver of the hour-circle seems about as near to perfection as it need to be. Such was the admirable means with which Johnson proposed to investigate the knotty problem of stellar parallax, on Bessel's plan; and I strongly advised him to adopt γ Virginis as one of his objects, it being at once eligible on account of its easy position, its comparative insularity, and the advantageous figure formed by its immediate *comites*.

But, while thus rendering my meed of admiration to the costly Oxford heliometer, I must still repose with confidence in the goodness and trustworthiness of the telescope I have so long, and, as I trust, pretty successfully, used: besides which, it was better adapted to my object and means. The difference of mounting between the two instruments will be instantly apparent to the most unpractised eye, by examining the plate of the equatoreal and the following representation of the heliometer, drawn by Mrs. Smyth—



A. Frame of the divided object-glass.
B. Microm. head of the external scale.
C. Cradle bearing the tube, which is turned by the handles *hh*, on the collars *cc*.
D. Position circle, whose slow motion depends on *ee*, while the clamp and tangent screws are *ff*.
E. Microm. microscopes for reading off the interior scale at the object end, illumined by galvanism.
F. Rod for separating the object-glass.
G. Iron box covering the declination axis.
WW. Friction rollers for relieving this axis.
H. Declination slow-motion handle, and *ss* its screws.
K. Rod for clamping the declination (the clamp is not seen.)
LL. Declination circle.
M. One of the reading microscopes of ditto; the instrument is roughly set by the wooden handles.
NN. Part of the course of the galvanic communication.
PP. Counterpoises for the declination and AR axes.
RR. Hour circle.
S. One of its microm. microscopes.
T. Clock for carrying the instrument.
t. Rod for setting the clock going.
u. Rod for regulating its rate.
x. Rod for connecting it with the hour circle.
y. Clock weight.
ZZZ. Box containing the polar axis.

To return to Hartwell. The equatoreal-room is furnished with a chronometer compensated for sidereal time, and having a stop second hand, with a leaping-spring, for observing occultations and the like phenomena. There is also a simple and sonorous "journeyman," which was constructed for the late Colonel Beaufoy, and can readily be put in beat with the transit-clock when required. The telescope adjuncts—as eye-pieces, micrometers, shades, fog-tubes, &c.—are the same which have been already described at page 153. The equatoreal clock-motion invented by the Rev. Richard Sheepshanks, and presented to me by him, is also fixed in this tower, with its governor, preventing the jerking or grinding, which a vibratory pendulum would inevitably occasion; for, as the force on the train increases the velocity of the balls of the governor, they expand, or further fly out, and thereby increase their moment of inertia or resistance to motion. Mr. Sheepshanks called the adaptation a "conical pendulum," because the rod of each ball describes a cone as it revolves: others designate it a "vertical pendulum,"—a case in which *utrum horum, &c.* is applicable.* It has lately been improved upon.

Having been questioned at Woolwich as to why I made no mention, in the Celestial Cycle, of Professor Barlow's fluid object-glass, although its properties had been referred by the Royal Society to Herschel, Airy, and myself, a word of enrolment may be necessary for the general inquirer as to the various suggestions for the advancement of telescopic astronomy. In the first place, I considered the experiment related rather to a temporary expedient than to a permanent instrument, nor was the principle new, though its modification was ingenious and highly creditable to the Professor. In the year 1832, however, when this inquiry was ordered, public expectation was rather extra-

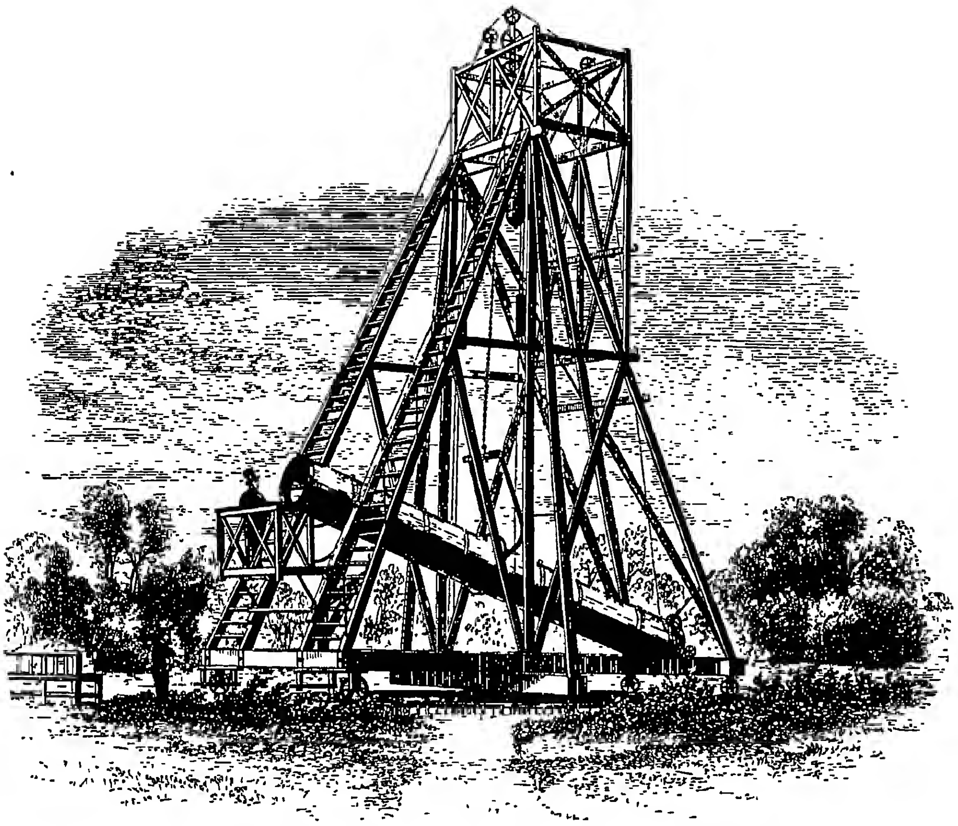
* A large and well-constructed model of this observatory, with its instruments, was sent by Dr. Lee to the grand Exposition of 1855, at Paris; after which it was presented, through the Abbé Moigno, to the Observatory in the Jardin des Plantes. The receipt of this present, displaying the astronomical establishment of an English gentleman, was courteously acknowledged by Mons. Le Verrier, of Neptunian renown, in a letter dated 13th of November, 1855. The model was the work of the village carpenters, George and Charles Carter, father and son; and the Equatoreal, Transit, and Clock were accurate reductions by George Dollond, at a cost of 46*l.* 10*s.*

vagant in anticipating fluid object-glasses of enormous diameter and small price; but the 8-inch one before us cost the Royal Society, under whose direction it was made, 157*l*. 10*s*. It will also be recollected that this scheme was brought forward just at the time when Faraday, whose time was surcharged with other matters, had declined to investigate the making of glass of a superior quality for optical purposes. "The best step," said he to me, "to ensure improvement will be to take off the Excise duty!" To satisfy curiosity on the point in question, I shall place the Official Report in the Appendix to this volume, to speak for itself.

With regard to the principle, it is difficult to assign the palm, since the information respecting it must be so scattered that it is no easy matter to trace the suggestions. It is now more than a century ago since John Dollond constructed an object-glass containing pure water, in hopes that equal and opposite refractions of the liquid and the glass would destroy colour; but, though the object seen through it was indeed free from colour, it was by no means so distinct as was expected, and consequently the spherical aberration yet remained. The trial however led to the adoption of other means, and they were followed by that beneficial result—the Achromatic Telescope—by which, and its being used by able men to the best advantage, the conceptions of mind have been advanced and enlarged. But the wish for a fluid object-glass was not abandoned. Mr. Benjamin Martin, the well-known mathematician and mechanic, whose contrivance for drawing out telescope tubes proved so useful to the Tulleys, is said to have constructed an instrument which, by the successful use of a peculiar fluid, succeeded better than that of his friend Dollond. This was about the year 1765, and certainly few men of that day could have been better versed in the doctrine of telescopic aberrations, as they relate to practice, than the industrious but unfortunate Martin. I am not aware whether this fluid-correcting lens is mentioned in any of the numerous philosophical works which he published, but the elder Tulley thought it was in existence in 1820. About the year 1787, Dr. R. Blair, then Professor of Astronomy in the University of Edinburgh, made many ingenious experiments with a view to ascertain

the dispersive powers of different liquids. On the strength of the result of these trials Mr. Robert Blair, a naval surgeon, took out a patent in 1791 for securing to himself the advantage to be derived from using a fluid medium in conjunction with glass, to correct the prismatic aberration in the object-glass of a refractor, agreeably to the researches of Dr. Robert Blair. Except a poor attempt by a friend of mine, the matter slumbered for nearly forty years; when attention and hope were suddenly attracted by Professor Barlow's application of the sulphuret of carbon, decidedly the best medium ever employed for such a purpose, as not having so much glare of false light, which so sadly interferes with the definition of most giant reflectors.

Since the Cycle was published, material steps have been made in the construction of efficient achromatic telescopes of superior size, before the power of which fresh wonders arise, and many of the abnormal phenomena disappear. Such are the concentrated forces drawn from the dark flint and the grains of sand; but it is well known that large metallic reflectors can be made more easily, and at less cost, than refractors of great aperture—the latter being extremely expensive, and therefore valued in proportion to the cubes of their diameters. A knowledge of this induced Lord Rosse to bestir himself in the cause of science; and as the noble Earl brought a large amount of mathematical and mechanical skill into the field, strengthened with a rare union of zeal and perseverance, his efforts were attended with complete success. His Lordship commenced with what is now termed his *small* telescope, that is, a speculum three feet in diameter; but this was many years ago, for it is over a quarter of a century since he described—and admirably described—the process then in hand, at my house in Bedford. The result of the enterprise was one of the best figured mirrors, and the most perfect reflecting instruments, hitherto produced; and the effect it had on the nebulæ, even those which had encountered the eagle-gaze and great power of Sir William Herschel, was truly astonishing. By the talent and taste of Lady Rosse, I am enabled to place its resemblance here; and, small as the figure in the observing gallery necessarily is, there will be no difficulty in identifying it:—



Between the times of Sir William Herschel and the Earl of Rosse, another peer of the realm had been walking the same course, or, as is most incorrectly, but very commonly said, moving in the same sphere. This was Charles, Earl Stanhope, a personage equally celebrated for mechanical inventions and scientific researches. Having heard from some of his contemporaries, long ago, that his Lordship had not only compassed, but actually commenced the making of a very large reflecting telescope, and being unable to gather accurate details elsewhere, I wrote to my friend the present Earl, the distinguished historian, for information on the point. His Lordship, as is usual with him, promptly answered my inquiry, from Chevening, 18th October, 1857, in these terms:—

It is quite true, as you have been informed, that my grandfather had intended to construct a new telescope upon a different principle, and of a much larger size than any which existed at that time. It was to have been made under his eye by Mr. Varley, a gentleman of some note in science, whom he retained in his employment, and in his house, with a yearly salary.

The design was, however, interrupted by his last illness. I do not know that any actual progress had been made in the work, beyond the acquisition of a large quantity of mahogany, which was found ready prepared for that object at his death.

His Lordship forwarded me some documents, selected from among his grandfather's papers on the subject, adding, "Pray also at the same time accept a small Centenary Almanac of his designing,* which you will likewise find inclosed." On inspecting the records thus entrusted to me, I found a letter addressed to the Rev. John North, of Ashdon, near Saffron Walden, in 1809, mentioning that he had discovered the way of obtaining in a reflecting telescope wider than long—say 36 inches aperture by 30 in length—great distinctness with a flood of light, though under high magnifying powers. This telescope moreover, when the object is placed where the image of a distant one is formed, would act as a powerful microscrope, and also prove a most powerful burning mirror; while, as a night-glass, it would be superior, because possessing fifty-times as much light as the best achromatic. In this letter he mentions his intention of erecting, at Chevening, the grandest telescope "ever beheld by the eyes of man!" And this is the design towards which my inquiry was bent, for I had heard of it from Mr. Rennie, Dr. Wollaston, Sir Humphry Davy, and Thomas Jones the mechanician, who all agreed that it was to have been a huge monster of enormous aperture. Of this, as impressed on my memory, I can find no trace; but in a letter from Chevening House, 28th January, 1816, to the same gentleman, who was also addicted to optics, the Earl says, "Your plan and mine are distinctly different, though some few of our ideas coincide," and he proceeds—

My telescope has no eye-hole; but only an eye-placer, two inches in diameter, 

* This Kalendar was invented by Charles, then Lord Mahon, in 1777; and it was continued from 1868 for another 100 years, by his great-grandson, Edward, now Lord Mahon, in his sixteenth year; a copy of which was also kindly forwarded for my acceptance.

to take in the whole eye, and it meets the cheek, &c. all round; being duly shaped for the purpose, so as to exclude all side-light. Here we agree, so far as to reject those absurd consumers of light and inflectors of rays, viz. the small eye-holes. I found it out many years ago, merely by looking in succession through holes having no lenses, such as—∘ ∘ ∘ ∘ ○ ○ ○ ○ ○, &c.

We differ as to the small metal, and the hole in the large one. I have no small metal, and no hole in my large one. By very curious and decisive experiments, and quite new, I have settled that the diameter of the speculum should be only about $\frac{1}{48}$th part, at most, of its focal distance, for high powers, where not mere light but great distinctness shall be required. My two telescopes—now about to be built on the exact plan of that already executed by my own workmen and myself at this place—will be for the concave speculum of 10 inches diameter in the clear, with 40 feet focus; and for the one of 30 inches in the clear, 120 feet focus. These instruments, notwithstanding their large size, will be managed with the utmost facility, upon the new plan that I have invented and executed. By a most unanswerable experiment I have proved, that, by the specific means which I use, a circular aperture of only three-quarters of an inch in diameter is capable of conveying sufficient light for a magnifying power of 300 times, which is the same as an increase of area of 90,000 times. I believe this is new. The light in my largest telescope will be 1,600 times greater!

And here the strenuous and successful exertions of my friend Mr. William Lassell must be noticed, for no one can have exhibited more earnestness of purpose, knowledge of whatever he took in hand, or readiness of general resource, than he has done: tethered with the cares of business, his scientific ardour remained undamped, and he has exhibited equal skill in making and in using his instruments. In 1840 he erected an observatory at Starfield, near Liverpool, of which the chief feature was a Newtonian reflector of 9 inches clear aperture, with a focal length of 112 inches; but, determined upon employing still larger means for his celestial researches, Mr. Lassell soon afterwards surprised the scientific world with another grand specimen of his handiwork, in a reflector of 2 feet in diameter and 20-feet focus—constituting the largest equatoreal instrument in the world. Unsatiated with the discovery of a new Saturnian satellite, the re-production of those of Uranus, the timely satellite of Neptune, and other happy results of his explorations with this telescope, our energetic astronomer soon contemplated the making of another of still larger dimensions, also for a parallactic mounting: and, shortly after I had visited his *two* observatories, he astonished me by a letter dated 3rd September, 1857, in which is this passage—

I have lately begun the construction of a 40-foot Equatoreal, generally on the plan of my 20-foot instrument—the mirror to be four feet in diameter, and the focus perhaps a foot or two short of 40 feet. To be temporarily erected and mounted here, but removed for observation to my old *locale* of St. John's Cavalier at Valetta, if the Governor will permit me to resume it. I propose to have no dome, but a sort of revolving tower, or colossal sentry-box, so contrived as to give me access to the eye-piece in all required positions of the telescope; and in the day-time to be prostrated over the tube, and form a shelter from the sun's rays.

The tube will be skeleton-fashion, only half its surface being solid, and therefore allowing the most perfect equilibrium of temperature, or other condition of the atmosphere, within and without. The speculum is to have 38 cells or recesses at the back cast in it, to afford fulcra for an efficient system of levers to prevent flexure at moderate altitudes. The principal frame-work of the mounting will be wrought-iron—boiler-plate—in principle of construction analogous to the Britannia Tubular Bridge. There will be no stone-work beyond the plain level platform on which the instrument is to be placed. This is mainly for the convenience of setting up and taking down, though certainly such a telescope cannot well be called *portable*.

From my experience of the skies of Malta, I cannot but hope that something more of the heavens may be revealed by a telescope which shall have twice the magnifying power of that which I took there, with the same quantity of light, and with—as I hope and believe—as perfect and correct a figure. In this undertaking labour, time, and money must be pretty largely consumed, and I anticipate a twelve-months' hard work will be required to accomplish it. Not much progress has yet been made beyond making models and some of the apparatus required for casting the speculum!"

This gigantic labour being most perseveringly persisted in, was successfully accomplished by the beginning of 1859; and the speculum—the face and back of which are respectively concave and convex portions of a sphere of about 76 feet radius—appears to be perfect in every respect: but such is its massiveness for motion as an equatoreal member, that a weight of more than ten tons has to be supported on three surfaces of very small area on which the motion must be obtained.

In the above allusions to Malta I should observe, that when Mr. Lassell communicated to me his determination to undertake so troublesome an enterprize as carrying his 24-foot telescope abroad—entirely on his own outlay from limited means—I endeavoured to persuade him to select Mount Ætna for his experiment; for I felt assured that I could get the *Casa Inglese*, 9,600 feet above the sea, prepared for him. To this he was nothing loath, but, on a further consideration of his time and means, Malta was deemed the most

eligible: and I cannot but here insert part of a letter which he wrote to me from Valetta, 22nd December, 1851, as showing the result of his interesting and unique mission:—

I seize the opportunity of again thanking you for the effective interest you took in the success of my expedition hither, by rendering me those facilities which have rendered my visit here a very pleasurable one. I have received the utmost kindness from every quarter; and those mechanical difficulties in the erection of the observatory and telescope, which I naturally anticipated, were almost annihilated by the prompt and efficient aid I have all along received from each of the Government authorities who could render me service.

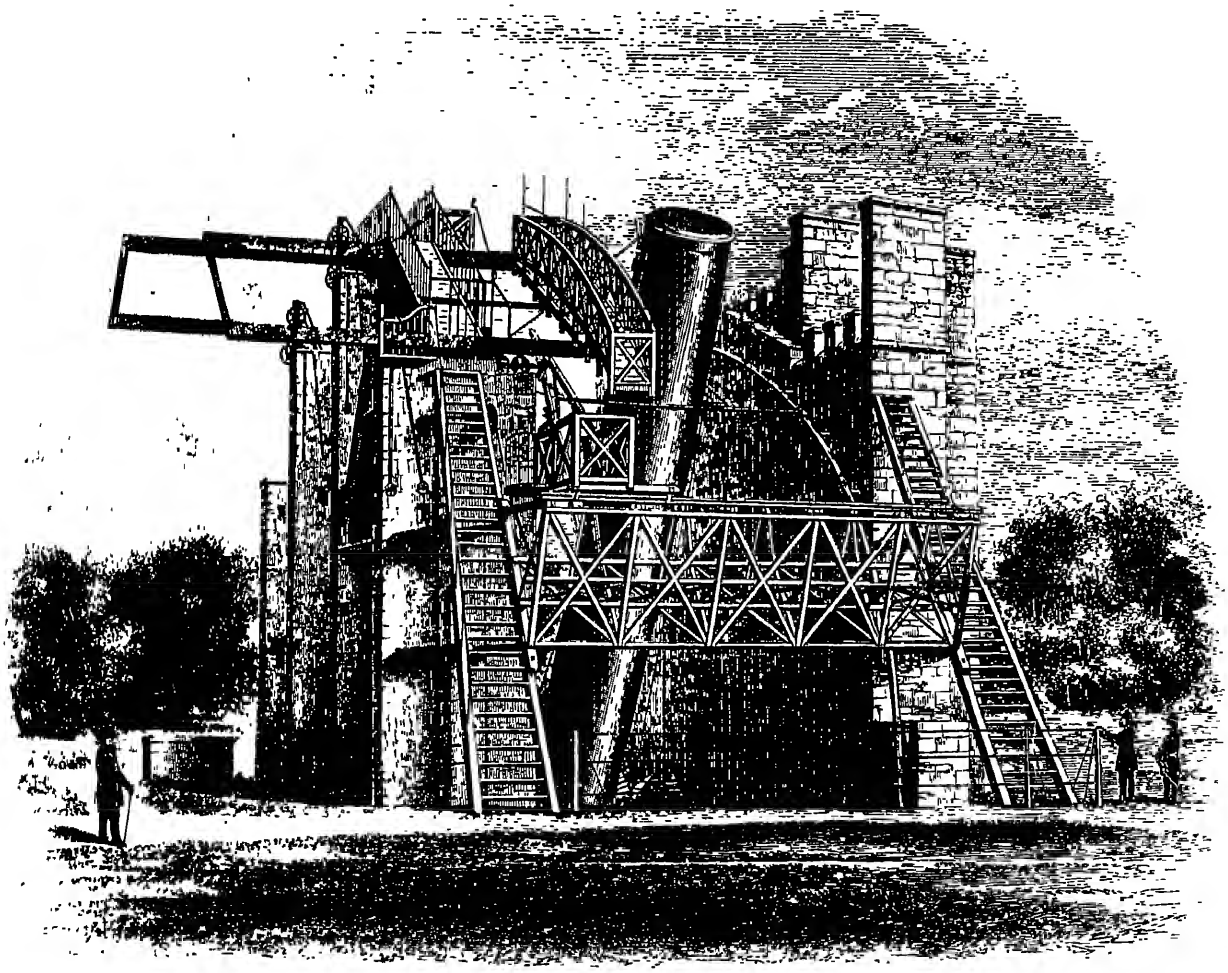
You are already aware how highly I am delighted with the climate, and how much more I can see here than I could at home. Certainly this is the place for an efficient observatory for all objects within a moderate south declination; for the quietude of the atmosphere, as you must have found, bears a large proportion even to its clearness and transparency. I here speak of the whole period of my observation until about a fortnight ago, when the weather became somewhat broken by thunder-storms and heavy rains. Still I have never found, on a clear night, that, even with the powers and aperture I commonly employ, the unsteadiness has been such as to prevent my doing useful work. The planets Neptune, Uranus, and Saturn, together with their satellites, have hitherto consumed so large a portion of my time, that I have had little to devote to other objects; but, as I have now nearly completed what I shall be able to do with them, I intend to survey some of the nebulæ and a few double-stars. I have indeed examined some of these with great astonishment and delight. Sirius is an object I would challenge any one to see as I see it, for the first time, without an involuntary exclamation of wonder! With 260, it is an incandescent diamond, which I cannot describe—but, if I must compare it with anything earthly, I should say it is an aggravation of the electric light. One would think its intrinsic brightness must surpass that of the Sun, to send out rays of such intense brilliancy to this all but infinite distance! The pencil of light transmitted through the eye-piece from this star, cast a brilliant spot on the wall of the observatory sufficient forcibly to attract my notice, without being at all prepared for it.

In the nebula of Orion, the first thing that struck me was the marvellous brightness of the fifth and sixth stars of the trapezium, which, notwithstanding their great difference of visibility in smaller telescopes, appear of equal magnitude in this, and have good-sized discs—by no means points. Perhaps a reason for the apparent inequality may be, that in smaller telescopes the closeness of the sixth star to the brightest of the group, hides its intrinsic brightness; while in mine it is sufficiently removed from any rays of the large star, to be judged of by its own merits. I was further surprised to find throughout the nebula *very few* more stars than I have been able by the rarest glimpses to make out at Starfield; though the stars there seen with so much difficulty are obvious here under ordinary circumstances. The bright nodules, as it were, of the nebula, which I almost expected to turn into clusters here, have no such appearance or even tendency that way. In other words, there is not, to my apprehension, the least condensation or approach to resolvability in the nebula. All the stars I see are individual, isolated, and rather unusually brilliant points, without apparently any connection with it. Examined under good circumstances, with power 1018, the brightest parts of

the nebula look like masses of wool, a good deal drawn out at the edges, so that the sky can be seen through; one layer seemingly lying partly over another, so as to give the idea of great thickness or depth in the stratum.

Meantime the persevering Earl of Rosse, not satisfied with the signal success which had followed his first achievement in practical optics, again betook himself to work in constructing the largest telescope that had ever yet courted the heavens; and by his inductive improvements in compounding the mirror-metal, and the treatment of it while cooling—as well as in grinding and polishing the figure—the whole was brought to a happy conclusion, though at an expense of upwards of 12,000*l*. It is a triumph of the noble peer and the age, as well in conception as in execution and application; but it is rather bold to pronounce that it has arrived at "the bounds beyond which the laws of matter forbid human ingenuity to pass": however, considering the amount of time, talent, devotion, and money that would be required for such a purpose, we may very safely predict that Halley's comet will return before we see it surpassed. This remarkable and mighty "optick tube" has a speculum six feet in diameter, with a reflecting surface of 4071 inches, and weighing upwards of three tons. Its focal length is 52 feet; but the tube—made of deal hooped with iron—is 56 feet long, including the speculum box, the whole weighing above 15 tons. It is fixed to a large universal joint, imbedded in solid masonry about six feet below the ground, which allows it to turn freely, and is elevated or depressed by a chain and windlass, with counterpoises in every direction. On each side of this tube, which is seven feet in diameter, in a line with the meridian, and at twelve feet distance, a stout wall is built—seventy-two feet long by forty-eight high on the outer side, and fifty-six on the inner; allowing a lateral movement of the telescope of half an hour on each side of the culminating line; that is, a term of one hour in the twenty-four feet interval from wall to wall. Every portion is an evidence of powerful mental and mechanical energy; but he who wishes to see γ Draconis pass the meridian, or other heavenly body in the zenith, must stand at an elevation of at least fifty feet from the ground.

Two views of this mammoth-instrument came into my possession; but as they differed in essential points, I applied to the fountain-head, well assured that Lord and Lady Rosse would grant my prayer. Nor was I mistaken, for shortly afterwards a couple of likenesses of the GRAND REFLECTOR came to hand, under convoy of a letter from Parsonstown, dated March 5th, 1858, in which his Lordship said, "I inclose two photographs. If they do not answer your purpose, as the season is improving we can easily provide you with something better. The only work out of the common way we have at present in hand, is a lattice tube for the three-foot telescope, and some little improvement in the adjustment of the photographic clock, prefatory to a renewed attempt to obtain photographs of the nebulæ." And here is as faithful a copy of her ladyship's picture of the giant, as Mr. Cobb's art can render.



CHAPTER V.

OF MR. EPPS'S MERIDIONAL OBSERVATIONS.

Here man explores Creation's wondrous laws,
That teach him to adore the GREAT DESIGNING CAUSE.

TOWARDS the end of the year 1837, Dr. Lee engaged the late Mr. James Epps as his astronomical assistant at Hartwell. This gentleman had been for more than eight years the respected Assistant-Secretary of the Royal Astronomical Society, discharging his duties with uniform urbanity and intelligence: and, as he had for many years previously had charge of the chronometer-rating of a large marine establishment, he was considered to be fully qualified for carrying out the Doctor's wishes with regard to the moon-culminating stars. All the preliminary arrangements having been made, Mr. and Mrs. Epps were accommodated in Hartwell House from January, 1838, to the 10th of August, 1839, when he was suddenly taken ill, and died in his sixty-second year. He was buried in Hartwell Church, where Dr. Lee has placed an appropriate tablet, with an inscription to his memory. It will therefore be seen that Mr. Epps was but a short time in his new situation: he entered upon its duties with ardour, and had so far acquired the esteem and regard of his patron, that Dr. Lee generously assigned a liberal pension to the widow.*

The geographical position of an observatory is always a matter of some

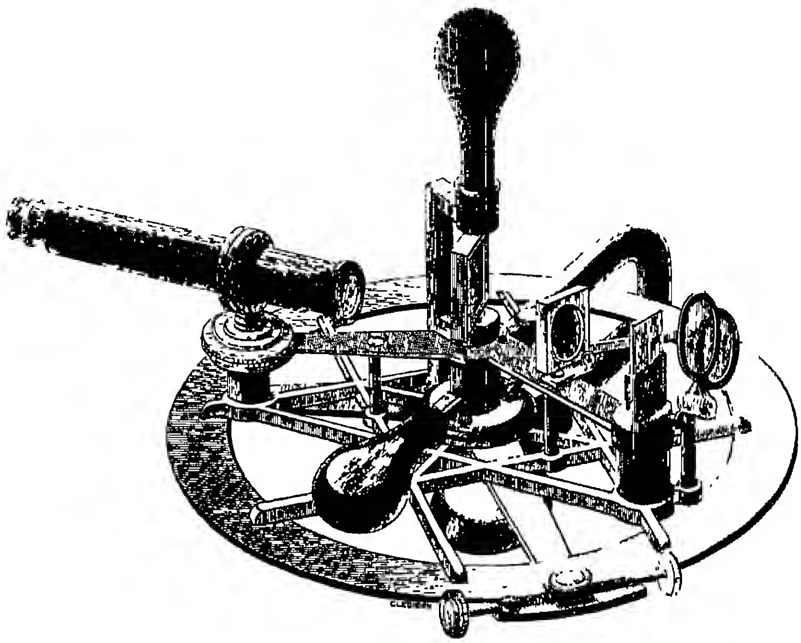
* In January, 1845, Mr. John Glaisher, brother of the well-known Assistant Astronomer at Greenwich, was partly engaged to continue the Hartwell observations: he had just entered upon the regular routine, when he was most unexpectedly attacked with illness, and died, at the early age of twenty-seven years, on the 16th of May, 1846. He had received his instruction under Professor Challis, at the Cambridge Observatory. His registered operations still await arrangement and reduction.

interest, although a rigidly accurate determination of the several co-ordinates may be actually necessary only in those public meridian establishments, where the absolute place of the moon has to be fixed. But, in compliance with the general rule, I had made a few observations for an approximate latitude and longitude so far back as April 1829; taking the altitudes of the Sun, Procyon, and Regulus with a ricketty reflecting circle and an artificial horizon for the first, and establishing the second by two trips with a pocket-chronometer from my clock at Bedford. These were the results—

Latitude		51°	$48'$	$35''\cdot6$	north.
Longitude	in arc . .		50	03	west.
	in time .		3^m	$20^s\cdot2$	+

By the term used above—ricketty reflecting circle—I must not be misunderstood, since no imputation is meant to be cast upon Troughton's beautiful instrument—an instrument capable of extreme precision: the epithet was intended only to apply to the state of neglect into which it had been allowed to fall, insomuch that cleaning, and all its possible corrections, had to be made before I could use it. In praising this circle, it will be recollected that those of Mayer and Borda were intended to compensate for imperfect division, by a system of repetition: but from the improved graduation, and inflexibility of frame, Troughton's Reflecting Circle is unquestionably an improvement on all precursors. It is true that its three indexes are inconvenient and tedious as to reading off, and that it does not repeat: but, where the angle is taken by a well-drilled hand and eye, the multiplication of observations is rendered unnecessary by the singular nicety of which it is capable—nor has it the embarrassing necessity, which is a condition in certain other instruments, of having previously to get the verniers set nearly to the apparent angle. As this noble tool eliminates errors of construction, reading, and even of principle, it leaves little to desire in a practised quarter: nor should it be forgotten that Don Joachim de Ferrer, an officer of the Spanish Navy, determined the obliquity of the ecliptic with one of Troughton's Circles,

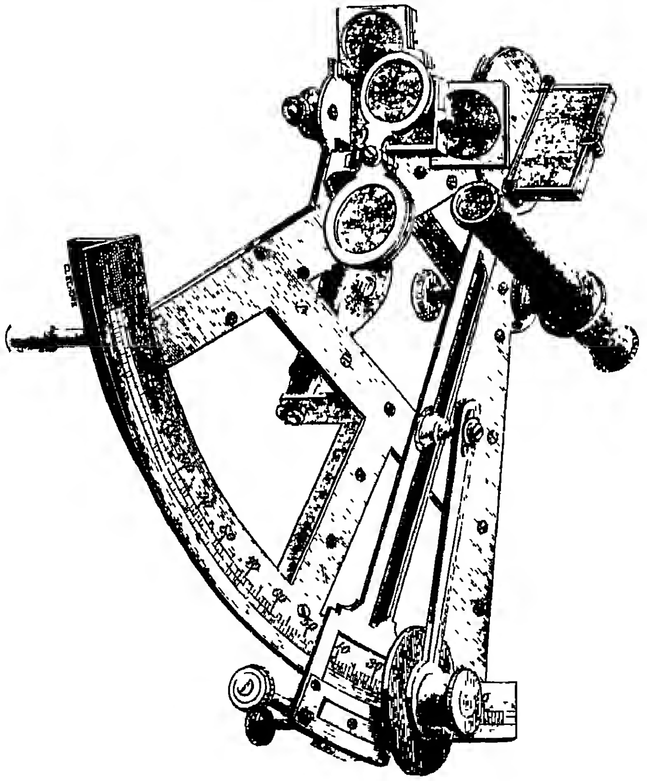
more accurately than the Greenwich large mural quadrant, then in use, could have done. Here is its portrait—



Now, if no degrading reflection was intended to be cast upon the Circle, still less are some of my remarks on the Sextant—elsewhere made—to be misinterpreted; my object in so expressing myself was merely to suggest that, good as it is, it will still admit of improvement; as in the use of silvered mirrors—wherein some of the rays of large angles of incidence are liable to distortions of refraction and reflection. This might be deemed hypercritical, but the main bearing of my warning was this—that, as observations of altitude afloat are capable of less accuracy than those made on shore, I recommended the use of a small alt-azimuth for travellers, instead of the sextant, with which moreover full dexterity in handling is indispensable. Yet I have known of one person, about to proceed on an inland mission, content with receiving only a lesson in the optician's shop where he had bought the instrument; and my friend Colonel Everest, while occupied on his grand meridional arc, was written to by a person travelling in India, to ask what was meant by 'index-error,' saying that otherwise he was quite up to the use of the sextant,

but there he was posed! Observations from such parties—equally defying discussion, arrangement, and even cooking—have sadly obstructed our map-makers.

But the sextant is a precious boon for mariners: to the happy discovery of measuring celestial arcs by the catoptric property of reflection, nautical astronomy is more greatly indebted than to any subsequent invention.* Under the two-fold adjustments of the maker and the observer, carefully made, and in a practised hand, it is a most powerful instrument for angular distances and altitudes; as I, from many years of constant use, can vouch for. It eminently combines lightness and strength, and therefore can be held in the hand while the ship is rolling, pitching, or scending; and the "artist" is able to measure his arc in a vertical, horizontal, or oblique direction. This is a representation of Ramsden's 10-inch sextant, mentioned at p. 123.



* Those Dons of "pure" science, who boast that they don't wish to be gaugers, often give themselves airs in respect to

Being satisfied that the above geographical conclusions would meet the existing wants, I rested on my oars: and, in correcting any given quantity of time from the ephemeris, I usually applied one-eighteenth of an hour west of Greenwich as suitable to Hartwell, and as sufficiently near for all probable exigencies.† But, when there was to be a regular observer employed, I suggested that a strong *fasciculus* of moon-culminators should be at once begun, for the two-fold purpose of settling the longitude at home, as well as of aiding others abroad. This was complied with, though not carried to the required extent: yet a paper was drawn up and read to the Royal Astronomical Society in 1839 which, having taken place while I was in South Wales, I did not happen to see the document. I understand, however, that it is somewhat anomalous in some of the clock-rate and azimuthal details, which probably require a further sifting. Still, most of the observations might, even in their present garb, be useful in a further series that may yet be made at Hartwell; for in those which Mr. Epps took for time, every attention was paid to the state of the instrument, namely, that it worked with no apparent error in collimation, and very little in level, but correcting for its azimuthal deviation as occasion might require. Except therefore the unavoidable errors of observation, and some trifling optical defects, it was concluded that nothing of importance could be urged against the mean of all the results.

Meantime Dr. Lee had a very long meridian-line marked out and accurately measured, which revived the question. This line commences near the windmill on Bledlow Ridge, runs due north through the transit-instrument at Hartwell, and trends onwards to Scots' Hill, near the ancient camp at Whitchurch.

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their more helpful comrades. Even while Galileo was finding work for geometry by instrumental means, our learned and munificent Sir Henry Saville, founder of two Oxford professorships, gave an illiberal rebuff to one of the most useful working mathematicians of that, or indeed any other time. Bishop Seth Ward tells us, that "he first sent for Mr. Gunter from London, to be his Professor of Geometry: so he came, and brought with him his sector and quadrant, and fell to resolving of triangles, and doeing a great many fine things. Said the grave knight, 'Do you call this reading of Geometrie? This is shewing of trickes, man!' and so dismissed him with scorn!" Poor Gunter! But Saville was right as regarded a teacher for a place of education.

† In the folio volume published by Adams in 1700, intituled "Index Villaris," the position of Hartwell House is placed in latitude 51° 49′ north, and longitude 0° 47′ west of London. Very fair for the time.

The whole is a length of about twelve miles (*see the map*), and a stout pole of forty feet in height was erected at each extreme: it was considered that from these stations, when their bearing and distance should be more accurately determined, the observatories of Oxford, Hartwell, Bedford, and Cambridge might be geodesically connected, and afterwards carried from thence to Greenwich, the intellectual starting-point of the empire. It so happened that, in the summer of 1842, my son Captain Henry Augustus Smyth, of the Royal Artillery, then a cadet in the Military Academy at Woolwich, accompanied me to Hartwell, whither I was repairing to re-measure some sidereal objects. On this occasion, the weather being very fine and having some leisure time, we made a correcting survey of the Hartwell grounds, and re-examined the long meridian-line. This was an opportune lesson for the youth; for, though the theory of surveying is tolerably attained at the Academy, still there are many little matters of application and instrumental adjustment, which are perhaps only obtainable in actual practice. It was therefore—to me—merely a renewal of old habits in which I had formerly indulged to a considerable degree, and a light course of field-performance for my son.

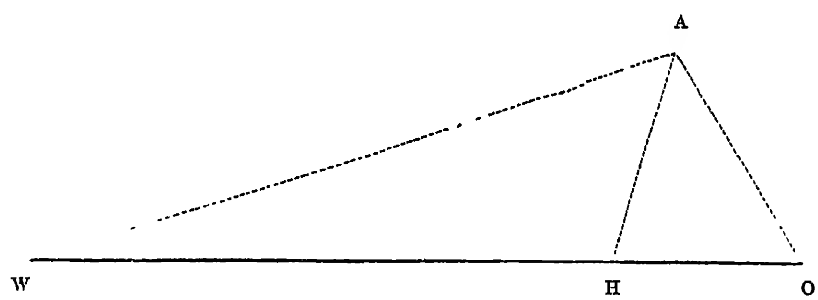
It now struck me, that as good a longitude for the Observatory as need be required, would be obtained by attaching it trigonometrically to Aylesbury Church-spire; the position of which in the great Trigonometrical Survey, as re-computed by Captain William Yolland, of the Royal Engineers, from the original data, is—

Latitude	. . . .	51° $49'$ $1''\cdot00$ north.
Longitude	arc . .	0 48 50 ·15 west.
	time . .	3^m $15^s\cdot34$.

In the first place, we carefully took the angles subtended between the principal objects of triangulation and the Whitchurch meridian-pole, with the seven-inch theodolite—carefully adjusted—standing on the roof of the transit-room, and plumbing the centre cones of that instrument. The mean of the readings thus obtained were—

Aylesbury Church-spire . . .	55°	35′	00″.
Ashridge Column	88	38	40.

Joined by Mr. Akehurst, who had made the first measurements, and Mr. William Blake, we then went to the north meridian-pole, where we had good and distinct vision of all the necessary points, even to Bledlow Ridge, had we wanted them. The distance from the Observatory to the Pole, as ascertained by Mr. Akehurst, is forty thousand two hundred and fifty-six links (0·66 to a foot); but I found it necessary, for the sake of commanding the angle, to measure back one hundred and fifty-six links on the meridian-line. My station was therefore forty thousand one hundred links from the Hartwell instrument, where I found the angle between the latter and Aylesbury spire was exactly 17° 31′; and from the station on Haydon Hill, ten thousand one hundred and forty-eight links from the Observatory on the same line, the angle was 73° 50′. The question now was simply

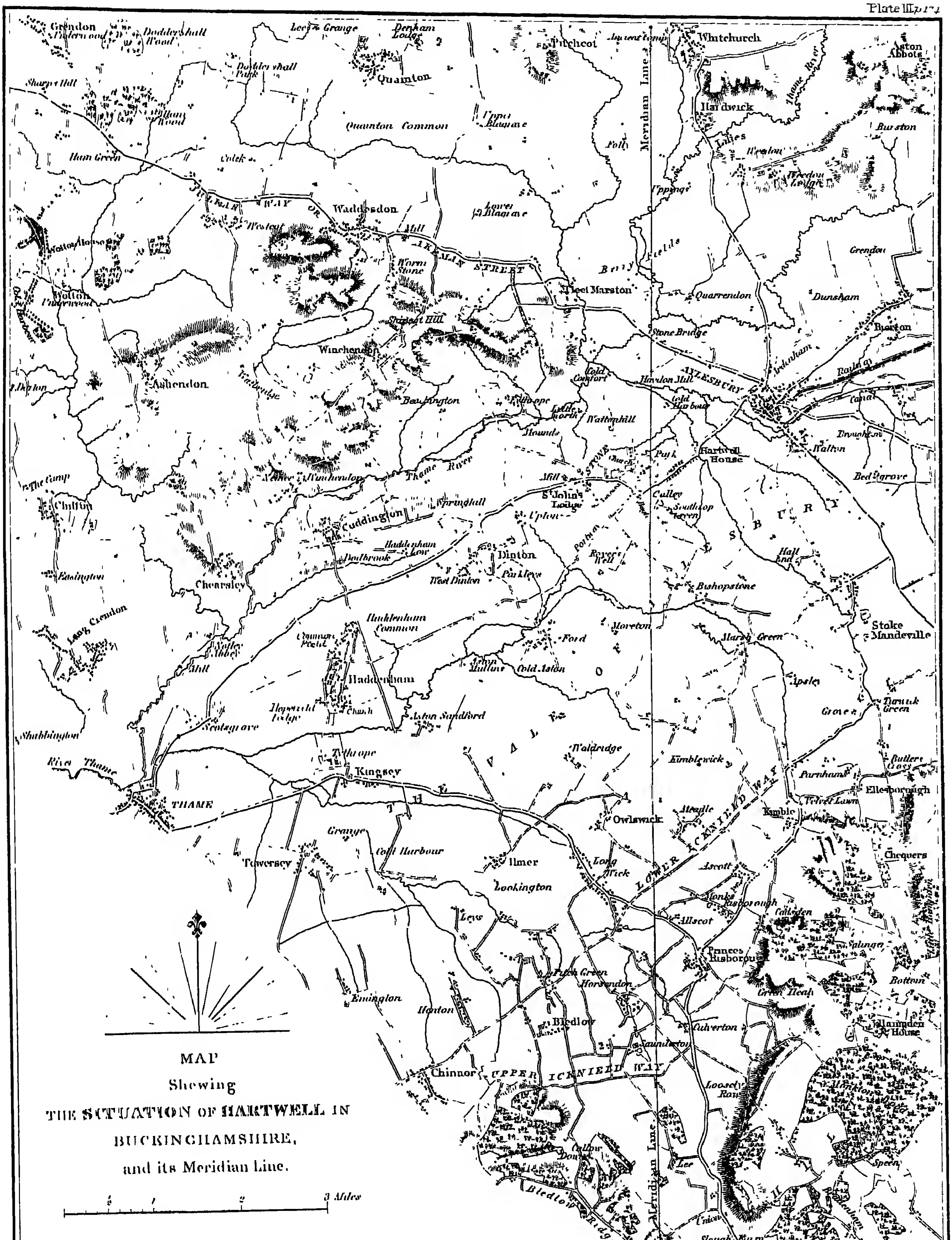


W, H, and O, being the base-line running from Whitchurch Pole, through the Haydon Hill station, to the observatory; and the apex is Aylesbury spire. We then have—

Sin. WAO : sin. AWO :: WO : AO = 4·1008594 or 12614·2 links.
Sin. WAO : sin. AOW :: WO : WA = 4·5387443 or 34573·6 links.
Sin. AHO : sin. AOH :: AO : AH = 4·0348092 or 10834·5 links.

With these data, and assuming that in this latitude 62.83 feet are equal

Plate III p 174



MAP
Shewing
THE SITUATION OF HARTWELL IN
BUCKINGHAMSHIRE,
and its Meridian Line.

3 Miles

to a second in space, it follows that the longitude of the Hartwell Observatory may be very safely regarded as in longitude 50′ 39″·46 west of Greenwich, or + 3^{m} 22^{s}·63 difference of time: and the latitude yielded by this method is 51° 48′ 14″·58 north. There is a vague rumour that General Roy's data for the position of Aylesbury spire require correction: when this shall have been ascertained and reduced to a fact, there is no doubt but the corrective quantity will follow the inquiry, and be duly applied; then the same amount may be added to or subtracted from the ordinate for Hartwell. But, until such an operation has been earnestly undertaken and satisfactorily performed, the trigonometrical longitude here given will answer every possible purpose, although mundane sphericity was not considered in the above computation.

To revert for a moment to the process by means of moon-culminating stars, I ought to have mentioned, that the vicinity of Hartwell is very strong in meridian means for attacking the problem, there now being no fewer than three other meridian batteries in immediate connection. The first of these is an excellent and efficient private observatory, erected by the Rev. J. B. Reade at Stone, and fitted with a fine transit-instrument on well-placed solid piers; and whose equatoreal telescope is worked under my original Bedford revolving dome. The second is a neat transit-room built by the Rev. Charles Lowndes, at the Hartwell Rectory; it is furnished with a transit-telescope of 4·2 inches aperture and six feet focal length, accompanied by a capital clock of Dent's. The third is a smaller one in Aylesbury, equipped by Mr. Thomas Dell. The last is well worthy of note, because it evinces the successful pursuit of practical astronomy under forbidding difficulties. Of less pretension than its costly neighbours, this Uranian room is but seven feet in length, five feet and a half wide, and six feet and a half high. It is constructed in the corner of a court-yard devoted to far different business, the turmoil of which has not prevented some excellent observations being made and recorded. By placing the pier for his thirty-inch transit and meridian-slit a good deal on one side of the room, Mr. Dell has contrived space for his clock and a writing-desk. The building is of wood, which, being screwed together, can be taken apart, easily carried off,

and set up again in a very short time, if necessary. The cost, including labour and materials, did not much exceed six pounds.

The Hartwell transit-instrument is already described; but, although not exactly necessary, it might have been interesting to mention that it resulted from a family legacy. The following inscriptions are engraved, on circular silver plates, above and below the transit cone's centre—

This instrument was made by
Thomas Jones of Charing Cross, under the inspection of Capt. W. H. Smyth, R.N.
For the Transit Room at Hartwell, 1831.

Joanni Lee, LL.D.
Testamento Legavit Louisa Soror Carissima,
A.S. MDCCCXXXI.

Mr. Epps principally observed those objects which transited the meridian, as the moon, moon-culminating stars, and planets: but, having something to learn in using instruments of a larger size than he had been accustomed to, he can hardly be said to have fairly entered upon what was proposed to be his standard occupation. There are therefore some deficiencies and various awkwardnesses in his recorded observations, for which he could no doubt in some measure have accounted, had he lived to reduce them himself. Four or five years after his regretted death, all his rough registers were forwarded to me at Chelsea; and, having closely investigated the whole of them, I selected three hundred and fifteen of his stars, many observed with the moon, by which to test the resulting right-ascensions, as a proof of their trustworthiness in questions of longitude. From the very tenor of this inquiry, it was attended with no small drudgery, as many were called that were not ultimately chosen. Every effort, however, was made to secure as great a number of the transits as possible from oblivion; but, owing to the instrumental corrections not being always noted, even the apparent clock-error was on many days too wavering to be depended upon. In this "fix," probability has claimed its

share of attention in selecting the Greenwich stars on which to append the corrections; and the squared sheets of papers mentioned in my Cycle (vol. i. page 429) were also called into requisition for describing the horological curve. In addition to which, a very efficient aid in these reductions was derived from computing a table of constants (sin. Z. D.—sin. P. D.) for every degree of polar distance, and applying the suitable correction to each star observed on that day; the azimuthal deviation constituting a most important element. In the following catalogue, the two Greenwich stars*—high and low—selected for this object, are given in a column assigned for that purpose. The detail of one day's sifting compared with the orthodox work will, perhaps, be the best explanation of the adopted system; since it was a case to which the more regular process of reduction was not altogether applicable.

By arranging the transits of each day in the order of their N.P.D. the increase or decrease of the clock-error shewed the rough azimuth deviation of the instrument; whilst the proportion applicable to each star was indicated by the table purposely prepared for the latitude of Hartwell.

Making due allowance for the lapse of time between the transits, and then applying it to the clock-errors, the difference between the high and low star is divided by the tabular azimuth difference between those two polar distances. The quotient thus obtained is multiplied by the tabular number, and the result is applied to the clock-error. Finally, the difference in this last column from the clock-error resulting from the selected index-star, indicates the modification required in the place before assigned to the unknown star. For example—

* The tyro may be reminded, that by the designation GREENWICH STARS is meant certain bright stars which, from their positions having been scrupulously ascertained by repeated observations with powerful meridian instruments, are made use of by all the working astronomers for the regulation of time, for latitudes, for lunar distances, and for station-pointers in celestial space. At my commencing acquaintance with them, they numbered 36; but in 1834 they were increased to 100, by a committee of which I had the honour of being a member. They have been current under the prenomina of Maskelyne's—Greenwich—Nautical—Known—Standard—and Clock stars.

1859. 2nd May.	Stars.	Approx. AR.	N.P.D.	Clock-error.	Diff. of AR.	Applied Clock-err.	Azimuth dev.	Applied Correction.	Corr. to star's place.	By usual method.	Azim. deviat. Table for Hartwell.
		h. m.	° ′	sec.	s.	sec.	s.	sec.	s.	s.	s.
comp.	δ Urs. Maj.	12, 07	32·02	+ 12·38	+ 0·19	12·57	— 0·10	+ 12·47	+ 0·65	+ 0·71	— 0·20
	γ Urs. Maj.	11, 45	35·25	+ 11·70	+ 0·17	11·87	— 0·04	+ 11·83	+ 0·01	+ 0·04	— 0·08
	α Can. Ven.	12, 49	50·49	+ 11·35	+ 0·22	11·57	+ 0·14	+ 11·71	— 0·11	— 0·03	+ 0·20
	Castor²	7, 24	57·46	+ 11·63	. .	11·63*	+ 0·19	+ 11·82	. .	— 0·06	+ 0·40†
	Pollux	7, 36	61·35	+ 11·72	+ 0·0	11·72	+ 0·22	+ 11·94	+ 0·12	+ 0·11	+ 0·45
	β Leonis	11, 41	74·32	+ 11·32	+ 0·17	11·49	+ 0·30	+ 11·79	— 0·03	+ 0·01	+ 0·62
comp.	π Virg.	11, 53	82·26	+ 10·98	+ 0·18	11·16	+ 0·34	+ 11·50	— 0·32	— 0·27	+ 0·70
	Procyon	7, 31	84·22	+ 10·99	. .	10·99	+ 0·35	+ 11·34	— 0·48	— 0·50	+ 0·72
comp.	γ Virg.	12, 33	90·30	+ 11·40	+ 0·21	11·61	— 0·38	+ 11·99	+ 0·17	+ 0·26	+ 0·80
	β Corvi	12, 26	112·30	+ 11·12	+ 0·20	11·32	+ 0·50	+ 11·82	. .	+ 0·09	+ 1·05

Castor 11·63*
·31 dividend.

·40†
Diff. ·65)·310(47·7
·500
·15

δ *Ursæ Majoris observed* 12h 7m 40s·82

a	b	c	d
—9·0992	—7·5818	+0·4776	—9·0275
—1·1434	—1·1330	+9·5461	—0·9611
+0·2426	+8·7148	+0·0237	+9·9886

Nat. num. + a 1·748
b 0·052
c 1·055 } +3·828
d 0·973

A.S.O. 12h 6m 57s·58 prec. 3·008
27 ·03 9
3 ·83 27·027
7m 28·44
Observed 7m 40·82
Clock + 12·38

Error by Procyon.

29 April	+ 14·20	·48	·48	·48	·48 *Quotient.*
2 May	+ 10·99	·2	·08	·3	·4
Lost in 72 hrs 3s·21 = ·04 per hr.		·096	·0384	·144	·192 *Az. Dev.*

Error by Pollux.

29 April	+ 14·63	·48	·48	·48	·48
2 May	+ 11·72	·46	·6	·7	·8
Lost in 72 hrs 2s·91 = ·04 per hr.		·216	·288	·336	·384 *Az. Dev.*

π *Virginis observed* 11h 52m 50s·32.

a	b	c	d
—8·8274	+7·3618	+0·4877	—7·9467
—1·1434	—1·1330	+9·5461	—0·9611
+9·9708	—8·4948	+0·0338	+8·9078

Nat. Num. + a 0·935 +2·096
c 1·080 } b —0·031
d 0·081
+2·065

A.S.O. 11h 52m 09s·61 prec. 3·074
27 ·06 9
2 ·07 27·666
39 ·34
Observed 50 ·32
Clock + 10·98

Error by Castor.
29 April + 14·59
2 May + 11·63
Lost in 72 hrs 2s·96 = ·04 per hr.

·48 *Quotient.*
1·05 Az. N.P.D. 112° 30′
·240
·480
·5040 Az. Dev. β Corvi.

γ *Virginis observed* 12h 33m 43s·50.

a	b	c	d
—8·8194	—7·9814	+0·4803	+6·7735
—1·1434	—1·1330	+9·5461	—0·9611
+9·9028	+9·1144	+0·0264	—7·7346

+a 0·918		+2·110
b 0·130	d	—0·225
c 1·062		+2·105

A.S.C. 12h 33m	02s·79	prec. 3·022
	27 ·20	9
	2 ·11	27·198
	32 ·10	
Observed	43 ·50	
Clock +	11 ·40	

Examples for correcting Æ. of unknown stars, by two Greenwich stars being assumed as known, α Cygni and α² Capricorni.*

Ex. I. γ Cygni gives clock-error too small; its assigned Æ being too small:—

4th July, 1839 . .	Computed . .	AR. 20h 16m 20s·23
	Correction	+ ·64
		29 ·87
	Observed	27 ·60
	Clock slow	2 ·27
	Azimuth deviation	— ·17
	The same as by α Cygni . .	— 2 ·10

Ex. II. θ Aquilæ gives clock-error too large; its assigned Æ being too large:—

4th July, 1839 . .	Computed . .	AR. 20h 3m 2s·72
	Correction	— ·15
		3 02·57
	Observed	2 59·97
	Clock-error	— 2·60
	Azimuth deviation . .	— ·50
	Agreeing with α Cygni .	— 2·10

In the following Catalogue, the Hartwell Stars are digested and tabulated upon a particular form and principle. The left-hand page contains the copy of Mr. Epps's registered observations, forming, as it were, the raw material of the right-hand or opposite page, where the results of reduction — by the method which I adopted—are given, and the difference of those results from the well-known Catalogue of the Royal Astronomical Society; the whole being brought up to the epoch 1850 by Mr. Baily's co-efficients. I have appended weights, 0 to 3, on the ultimate determinations, as shewing the comparative value which an investigation of them produced.

* The division of *known* and *unknown* stars is rather arbitrary in astronomical parlance: the first is a loosely-relative expression to distinguish between those bodies which have been so accurately determined as to become rated standard stars, while the latter name is applied to those which have been less examined. Some of these, however are nearly as excellently placed; and, as was the case with my late friend Sir Walter Scott, may be termed the Known Unknown (*see ante*, page 177, *note*). Indeed, many of them might be very safely enrolled in the columns of the Standard Catalogues.

OBSERVATIONS.

Date.	Star's name.	Telescope wires. I.	II.	Centre.	IV.	V.	Mean Transit.	Clock's Error.	Clock's Daily rate.	Remarks.
		secs.	secs.	h. m. s.	secs.	secs.	s.	s.	s.	
1838 3 Oct.	21 Andromedæ N.A. α	19·5	40·5	0 0 1·5	22·5	43·6	1·52	—03·32	—2·41	Rate with α Coron. 5 days before.
29 „	Ditto	21·0	42·0	0 0 3·3	24·2	45·4	3·18	—01·63	—2·08	Rate with ζ Aqui. 3 days before.
3 Oct.	88 Pegasi N.A. . . . γ	15·2	34·4	0 4 53·8	13·0	32·2	53·72	—03·77	—2·38	Level W. + 0″·96. Rate with γ Aqui. 3 days before.
29 „	Ditto	16·9	36·0	0 4 55·2	14·4	33·6	55·22	—02·26	—2·29	Level next day E. + 0″·55 Rate with γ Aquilæ 3 days previous.
3 Oct.	8 Ceti *comp.* . . . . ι	31·9	50·8	0 11 09·9	28·8	47·7	9·82	—03·86	—2·34	Level on the 5th red. to W. + 0s·61. Rate by α² Capr. 9 days after.
29 „	Ditto	33·6	52·5	0 11 11·2	30·0	49·0	11·26	—02·41	—2·17	Rate with α Capric. 3 days previous.
3 Oct.	44 Piscium . . . . ι	28·0	46·6	0 17 05·2	23·8	42·5	05·22	—03·84	—2·19	Wrottesley corrects N.A. by + 0″·41. Rate with α² Capric. 6 days previous.
29 „	Ditto	29·8	48·5	0 17 07·0	25·6	44·0	6·98	— 02·51	—2·21	Rate with α Capric. 3 days previous.
26 Nov.	Ditto	42·0	0·7	0 17 19·3	37·9	56·5	19·28	+10·35	—0·79	Rate with α Aqua. 2 days previous.
26 Nov.	18 Cassiopeiæ N.A. . α	29·0	2·0	0 31 35·0	8·0	41·0	35·00	+10·39	—1·16	Rate by γ Ursæ M., S. P. 2 days previous.
1839 17 April	Ditto S.P.	49·3	22·0	12 30 55·0	28·1	1·0	55·08	—27·59	—1·13	Rate with γ U. M. the previous day. Cloudy; mer. mark trem.
1838 26 Nov.	16 Ceti *N. A.* . . . β	1·5	21·1	0 35 40·7	0·2	19·8	40·66	+ 09·66	—0·35	Rate with δ Capr. 2 days previous.
„	P.189 Piscium A.S.C. 79	29·7	48·2	0 40 06·8	25·4	44·0	6·82	+12·08	—0·16	Wrottesley corrects it by + 1s·85. Rate with ζ Peg. 2 days previous.
1 Nov.	5 Arietis *N. A.* . . . γ	55·4	15·0	1 44 34·6	54·2	13·9	34·62	—08·26	0	Dble star, same AR.
1 Nov.	48 Arietis *comp.*. . . ε	13·7	33·5	2 40 53·8	13·1	33·0	53·32	—08·31	—2·01	Rate by γ Peg. 3 days previous. Az. er. by α Cap. and α Cyg. + 1″.28 and by α Ceti and α Pers. + 0·97.
27 Dec.	Ditto	40·4	9·1	2 50 29·0	48·9	8·9	29·06	+27·25	0	A good night's work. Clk. put forward to-day.
1 Nov.	92 Ceti *N. A.* . . . α	6·7	25·2	2 53 43·8	2·4	21·0	43·82	—09·02	—2·15	Rate with α Aqua. 3 days previous.
1 Nov.	Arietis . . . . δ	38·8	58·6	3 02 18·3	38·0	57·8	18·30	—08·14	—1·96	Rate with γ Peg. 3 days previous.
2 „	Ditto	36·8	56·4	3 02 16·0	35·8	55·5	16·10	—10·36	—2·02	Rate with γ Peg. 4 days previous. Level W. + 0″·25.
27 Dec.	Ditto	14·6	34·2	3 02 54·0	13·7	33·2	53·94	+27·24	0	The clock was put forward to-day. Az. E. + 0″·27 by α Aqui. and α Cyg. by Epps.

DEDUCTIONS.

Star's name.	AR. 1 Jan. 1850.	Approx. P.D.	Δ Ast. C.	Daily Axim. Dev. E.	Stars compared for daily azim.	Wt.	Az. correction.	Remarks.
	h. m. s.	° ′	s.	s.			s.	
21 Andromedæ . . α	0 00 37·93	61 45	+ 0·06	1·00	α Lyræ and mean	1	— 0·46	Level W. + 0″·96.
Ditto	„	„	— 0·73	1·28	α Cyg. and α² Capric.	2	— 0·58	Resumed eye-piece No. 3. Tolerable night.
88 Pegasi . . . . γ	0 05 30·86	75 39	— 0·21	1·00	α Lyr. and mean	1	— 0·63	Greenw. stars irregular.
Ditto	„	„	— 0·04	1·28	α Cyg. and α² Capric.	3	— 0·81	Encke's comet seen.
8 Ceti . . . . . . ι	0 11 46·78	99 39	— 0·02	1·00	α Lyr. and mean	1	— 0·90	Clouding over.
Ditto	„	„	+ 0·14	1·28	α Cyg. and α² Capric.	3	— 1·10	Level next day E + 0″·55.
44 Piscium . . . ι	0 17 42·45	88 53	— 0·12	1·00	α Lyræ and mean	1	— 0·77	Results unsatisfactory.
Ditto	„	„	— 0·10	1·28	α Cyg. and α² Capric.	2	— 0·98	Resumed eye-piece No. 3.
Ditto	„	„	+ 0·13	0·31	γ Drac. and Fomal.	2	+ 0·23	Transits irregular.
18 Cassiopeiæ . . α	0 32 00·70	34 17	+ 0·10	0·31	γ Drac. and Fomal.	3	— 0·04	Level W. + 0″·50.
Ditto S.P.	„	„	+ 0·23	0·50	γ Urs. M. and β Corvi	2	— 0·06	The lower passages are irregular.
16 Ceti . . . . β	0 36 03·19	108 49	— 0·47	0·31	γ Drac. and Fomal.	0	+ 0·32	Clear weather.
P. 189 Piscium A.S.C. 79	0 40 28·36	85 29	+ 1·85	0·31	γ Drac. and Fomal.	3	+ 0·22	The N. A. continued the error.
5 Arietis . . . γ	1 45 18·26	71 27	+ 0·56	1·35	α Pers. and α² Capric.	3	— 0·78	Too great an az. deviation. Epps gives two deviations for to-night.
48 Arietis . . . . ε	2 50 38·32	69 16	+ 0·48	0·21	α Cyg. and α Aqui.	1	+ 0·11	A fine clear night.
Ditto	„	„	+ 0·62	1·35	α Pers. and α² Capric.	2	— 0·74	Some Greenwich stars irregular.
92 Ceti . . . . α	2 54 26·36	86 30	+ 0·20	1·35	α Pers. and α² Capric.	3	— 0·75	Variable night.
Arietis . . . . δ	3 03 03·19	70 51	+ 0·86	1·35	α Pers. and α² Capric.	0	— 0·77	Meridian position same as yesterday.
Ditto	„	„	+ 0·17	1·20	α Pers. and γ Erid.	2	— 0·68	Instrument steady.
Ditto	„	„	+ 0·49	0·21	α Cyg. and α Aqui.	3	+ 0·11	Therm. in clk. case 36°.

OBSERVATIONS.

Date.	Star's name.	Telescope wires. I.	II.	Centre.	IV.	V.	Mean Transit.	Clock's Error.	Daily rate.	Remarks.
		secs.	secs.	h. m. s.	secs.	secs.	s.	s.	s.	
1839 15 July	38 Persei *N. A.*, S. P. α	. .	2·15	15 12 50·0	18·5	47·0	50·00	— 02·96	0	Rev. axis, no er. in collim.
1838 28 Dec.	Pleiadum *b*, or 17 Tauri	6·8	27·0	3 35 47·2	7·5	27·7	47·24	+ 26·95	— 0·53	Rate with ι Arietis yesterday.
2 Nov.	25 Tauri . . . . η	5·6	25·8	3 37 46·0	6·3	26·5	46·04	— 09·96	— 1·93	Rate with γ Peg. 4 days previous.
27 Dec.	Ditto	43·2	8·6	3 38 23·9	44·0	4·3	23·80	+ 27·35	0	The clock was put forward to-day.
28 „	Ditto	42·7	2·8	3 38 23·0	43·3	3·6	23·08	+ 26·63	— 0·72	Rate by η Tau. the previous day.
1839 25 Feb.	34 Eridani . . . γ'	35·4	54·4	3 50 13·5	32·6	51·7	13·52	— 18·17	— 1·35	Rate with ι Can. M. 2 days previous.
1838 2 Nov.	Tauri *comp.* . . . A'	21·5	41·4	3 55 01·4	21·2	41·5	1·40	— 10·21	— 1·90	Rate with ι Ariet. 1 day previous.
27 Dec.	Ditto	50·5	19·7	3 55 39·5	59·7	19·8	39·64	+ 27·48	0	Clock put on to-day.
28 „	Ditto	59·0	18·9	3 55 39·0	58·9	19·0	38·96	+ 26·80	— 0·68	Rate with itself yesterday.
1830 25 Feb.	14 Draconis *N.A.* S.P. η	13·2	52·8	4 31 32·2	11·8	51·0	32·20	— 17·70	— 1·27	Rate with η Drac. S. P. 8 days previous.
24 April	87 Tauri *N. A.* . . . α	23·0	42·4	4 27 01·8	21·0	40·3	1·70	+ 20·36	0	Rate uncert. Axis reversed.
25 Feb.	Ditto	. .	. .	4 26 24·0	48·0	2·5	23·83	— 18·15	— 1·67	Rate with ι Gem. 2 days previous.
1838 28 Dec.	102 Tauri *comp.* . . . ι	16·9	36·7	4 53 56·6	16·5	36·5	56·64	+ 26·70	— 0·65	Rate with η Tau. yesterday.
1839 24 April	13 Aurigæ *N. A.* . . α	15·5	42·0	5 05 08·6	35·2	2·0	8·66	+ 20·50	0	Level W. + 0″·11. Revd. axis on 22d instant.
„	19 Orionis *N. A.* . . β	. .	49·3	5 07 08·2	27·2	46·0	8·23	+ 20·03	0	Stars tremulous. Axis revd. on 22d instant.
1838 7 May	112 Tauri *N. A.* . . β	. .	58·7	5 16 19·9	40·8	2·0	19·80	+ 16·79	0	Axis reversed. Level W. + 0″·40.
28 Dec.	Ditto	53·0	14·0	5 16 35·0	56·0	17·2	35·04	+ 26·45	— 0·90	Rate with η Tau. yesterday.
2 Dec.	136 Tauri *comp.* . . . C	32·8	53·7	5 43 14·8	35·7	56·7	14·74	+ 01·06	— 1·45	Level E. 0″·71. Rate with α Peg. 8 days previous.
2 Dec.	58 Orionis *N. A.* . . α	51·1	10·0	5 46 28·7	47·3	6·2	28·66	+ 00·22	— 1·52	Rate with ζ Peg. 8 days previous. Star large. Level E. 0″·71.
1839 24 April	Ditto	10·4	29·0	5 46 47·8	6·7	25·2	47·82	+ 20·11	0	Very trem. clock put forward.
1838 2 Dec.	18 Geminorum *N. A.* μ	34·7	54·7	6 13 14·9	34·9	54·9	14·82	- 00·62	— 1·50	Stars difficult. Level E. + 0″·71. Rate with α Peg. 8 days previous.
1839 23 Feb.	27 Geminorum *comp.* ε	. .	27·8	6 34 48·2	8·8	29·3	48·25	— 14·81	— 1·79	Level W. + 0″·49. Rate with δ Gem. next day.
24 „	9 Canis Maj. *N. A.* α	8·8	28·2	6 37 47·5	7·0	26·3	47·56	— 16·79	— 1·32	Star bad fig. Rate by ι Can. M. previous day.

DEDUCTIONS.

Star's name.	AR. 1 Jan. 1850.	Approx. P.D.	△ Ast. C.	Daily Azim. Dev. E.	Star's compared for daily Azm.	Wt.	Az. correction.	Remarks.
	h. m. s.	° ′	s.	s.			s.	
33 Persei S.P. . . . α	3 13 37·77	40 41	+ 0·66	0·49	γ Drac. and μ Sagitt.	0	— 0·75	Transits irregular.
Pleiadum b, or 17 Tauri	3 35 58·27	66 20	+ 0·71	0·43	Capella and Rigel	0	+ 0·23	Level carefully taken W. + 0″·47.
25 Tauri . . . . η	3 38 34·03	66 20	+ 0·54	1·20	α Pers. and γ Erid.	2	— 0·61	Level nearly as yesterday
Ditto	"	"	+ 0·61	0·21	α Cyg. and α Aqui.	2	+ 0·10	Clock vibrations down to 16·0.
Ditto	"	"	+ 0·38	0·43	Capella and Rigel	3	+ 0·22	Level W. + 0″·47 Shutter open.
34 Eridani . . . γ	3 51 01·70	103 56	0	0·50	η Drac. S. P., and γ Erid.	0	— 0·47	Merid. position steady.
Tauri . . . . A[1]	3 55 49·48	68 20	+ 0·34	1·20	α Pers. and γ' Erid.	3	— 0·65	Level + 0″·25. Epps' Az. dev.=1s·23.
Ditto	"	"	+ 0·76	0·21	α Cyg. and α Aqui.	1	+ 0·11	A fine clear night. Thermometer 36°.
Ditto	"	"	+ 0·57	0·43	Capel. and Rigel	3	+ 0·24	Level W. + 0″·47.
14 Draconis S.P. . . η	4 21 57·32*	28 09	+ 0·20	0·50	η Drac. and γ' Erid.	2	— 0·98	The S. P. transits uncertain.
87 Tauri . . . . α	4 27 18·55	73 48	+ 0·12	0·64	Capel. and Rigel	3	+ 0·39	A long and good night's work.
Ditto	"	"	0	0·50	η Drac. and γ' Erid.	3	— 0·30	A merid. mark exact.
102 Tauri . . . . . ι	4 54 07·52	68 38	+ 0·50	0·43	Capel and Rigel	1	+ 0·24	Levelled with the roof open.
13 Aurigæ . . . . α	5 05 36·35	44 10	+ 0·14	0·75	γ Urs. M. and β Corvi	2	+ 0·10	A good night's work.
19 Orionis . . . β	5 07 19·65	98 23	— 0·02	0·64	Capel and Rigel	2	+ 0·55	Tried sev. pairs for Azim.
112 Tauri . . . . β	5 16 48·55	61 31	+ 0·37	1·00	Mean and Spica	1	+ 0·46	Level W. + 0″·40.
Ditto	"	"	+ 0·22	0·43	Capel. and Rigel	3	+ 0·19	Epps' az. dev. by same stars=0s·42
136 Tauri C	5 43 53·74	62 26	+ 0·38	1·47	ε Gem. and Sirius	0	+ 0·66	Star too large.
58 Orionis . . . α	5 47 03·00	82 38	— 0·01	1·47	ε Gem. and Sirius	0	+ 1·11	Tolerable figure.
Ditto	"	"	0	0·64	Capel and Rigel	1	+ 0·46	Level W. + 0″·11.
13 Geminorum . . μ	6 13 52·65	67 25	+ 0·07	1·47	ε Gem. and Sirius	0	+ 0·76	Irreconcileable with α Orionis.
27 Geminorum . . . ε	6 34 41·75	64 44	+ 0·29	0·42	Castor and ε Can. M.	2	— 0·21	Clouds flitting past.
9 Canis Maj. . α	6 38 32·13*	106 30	+ 0·03	0·24	α[2] Cast. and ε Can. M.	1	— 0·23	Variable night.

OBSERVATIONS.

Date.	Star's name.	Telescope wires. I.	II.	Centre.	IV.	V.	Mean Transit.	Clock's Error.	Daily rate.	Remarks.
		secs.	secs.	h. m. s.	secs.	secs.	s.	s.	s.	
1838 2 Dec.	42 Geminorum *comp.* ω	56·8	17·2	6 52 37·6	58·0	18·4	37·60	+ 0·97	—1·45	Level E + 0″·71. Rate with α Peg. 8 days previous.
1839 23 Feb.	55 Geminorum *N.A.* δ	36·8	56·8	7 10 16·8	36·9	56·9	16·84	— 15·11	—1·54	Level W. + 0″·49. Rate by α Gem. 2 successive days.
24 „	Ditto	35·8	55·2	7 10 15·3	35·4	55·5	15·34	— 16·60	—1·50	Rate with itself previous day.
23 Feb.	60 Geminorum *comp.* ι	48·0	9·0	7 15 30·1	51·3	12·1	30·10	— 15·01	—1·49	Very unsteady. Rate by itself the next day.
24 „	Ditto	. .	7·6	7 15 28·6	49·6	. .	28·6	— 16·50	—1·49	Rate by itself yesterday.
20 April	66 Geminorum *N.A.* α^2	4·0	26·0	7 23 48·0	10·0	32·0	48·00	— 32·29	—1·57	Very steady. Rate by Procyon yesterday and to-day.
29 „	Ditto	50·8	12·8	7 24 34·7	56·7	18·7	34·74	+ 14·59	—1·24	Rate with itself one day. Very steady.
1838 7 May	Ditto	48·5	10·4	7 24 32·3	54·4	16·5	32·42	+ 16·68	—0·95	Rate with itself 3 days after. Large power eye-piece No. 4 now applied.
1839 23 Feb.	10 Canis Minoris *N.A.* α	1·5	20·2	7 30 38·9	57·5	16·2	38·86	— 15·10	—1·49	Star very faint. Rate by δ Gem. 2 successive days.
24 „	Ditto	59·4	18·0	7 30 36·7	55·4	14·0	36·70	— 17·26	—2·16	Star very faint. Rate by itself yesterday.
16 April	Ditto	49·2	8·0	7 30 26·7	45·5	4·0	26·68	— 20·58	—1·80	Tolerably steady. Rate with Sirius 5 days previous.
19 „	Ditto	44·9	3·6	7 30 22·1	40·7	59·5	22·16	— 31·05	—1·49	Very steady. Rate by itself 3 days.
20 „	Ditto	43·1	1·9	7 30 20·7	39·3	57·9	20·58	— 32·62	—1·57	Rate by itself yesterday.
29 „	Ditto	30·0	48·7	7 31 07·3	25·8	44·6	7·28	+ 14·28	—0·87	Clock put forward 22nd. Rate with Spica yesterday.
2 May	Ditto	26·7	45·2	7 31 04·0	22·8	41·4	4·02	+ 10·99	—1·10	Rate with itself 3 days previous.
5 „	Ditto	23·7	42·3	7 31 01·0	19·8	38·4	1·04	+ 8·08	—0·99	Rate with itself 3 days previous. Large power eye-piece No. 4 now applied.
23 Feb.	78 Geminorum *N.A.* β	32·0	53·2	7 35 14·4	35·6	56·8	14·40	— 14·94	—1·15	Faint through clouds. Rate with Aldeb. 6 days previous.
24 „	Ditto	30·7	51·7	7 35 12·8	33·9	55·0	12·82	— 16·51	—1·57	Rate by itself yesterday.
6 April	Ditto	36·9	58·0	7 35 19·0	40·1	1·3	19·06	— 9·67	—1·17	Pretty good. Rate with Reg. 5 days previous when clock was put forward.
16 „	Ditto	20·5	41·5	7 35 02·6	23·7	44·8	2·62	— 25·93	—1·63	Rate by itself 10 days previous. Star faint.
19 „	Ditto	16·0	37·0	7 34 58·0	19·0	40·3	58·06	— 30·44	—1·50	Clear and steady. Rate with itself 3 days previous. Level E. + 0″·42
20 „	Ditto	14·2	35·4	7 34 56·6	17·7	38·8	56·54	— 31·95	—1·51	Rate by itself yesterday.
29 „	Ditto	0·9	21·9	7 35 42·9	4·0	25·2	42·98	+ 14·63	—1·06	Clock put forward on 22d. Rate with Arcturus 2 days previous.
2 May	Ditto	57·9	19·0	7 35 40·0	1·0	22·2	40·02	+ 11·72	—0·97	Rate with itself 3 days previous.
5 „	Ditto	54·9	16·0	7 35 37·0	58·1	19·2	37·04	+ 8·77	—0·98	Rate by itself 3 days previous.

DEDUCTIONS.

Star's name.	AR. 1 Jan. 1850.	Approx. P.D.	Δ Ast. C.	Daily Azim. Dev. E.	Stars compared for daily Azim.	Wt.	Az. correction.	Remarks.
	h. m. s.	° ′	s.	s.			s.	
42 Geminorum . . ω	6 53 15·93	65 34	+ 0·45	1·47	ι Gem. and Sirius	0	+ 0·75	Agrees best with O Tauri.
55 Geminorum . . δ	7 11 09·44	67 45	+ 0·14	0·42	α² Cast. and ι Can. M.	2	— 0·24	Clouds flitting past.
Ditto	,,	,,	+ 0·15	0·42	α Ditto	1	— 0·13	Haze increasing.
60 Geminorum . . ι	7 16 24·33	61 55	+ 0·19	0·24	Ditto	2	— 0·19	Very unsteady.
Ditto	,,	,,	+ 0·23	0·24	Ditto	1	— 0·11	Uncertain by clouds.
66 Geminorum . . α²	7 25 01·41	57 47	+ 0·08	0·81	ι Urs. M. and α Hydr.	3	— 0·32	Very steady.
Ditto	,,	,,	+ 0·20	1·00	η Urs. M. and Spica	2	+ 0·40	Also very steady.
Ditto	,,	,,	+ 0·26	1·00	Mean and Spica	1	+ 0·40	A troublesome night.
10 Canis Minoris . α	7 31 20·71	84 24	+ 0·23	0·42	α² Cast. and ι Can. M.	2	— 0·30	Level W. + 0′·49.
Ditto	,,	,,	— 0·45	0·24	Ditto	0	— 0·17	This the only bad transit to-night.
Ditto	,,	,,	— 0·20	0·30	γ Ceph. S.P. and β Corvi	0	— 0·14	An irregular night.
Ditto	,,	,,	— 0·15	0·45	γ Urs. M. and β Corvi	2	— 0·32	A good night's work.
Ditto	,,	,,	+ 0·01	0·81	ι Urs. M. and α Hydr.	3	— 0·58	Only Pollx out of bounds.
Ditto	,,	,,	+ 0·13	1·00	η Urs. M. and Spica	3	+ 0·72	A long series.
Ditto	,,	,,	— 0·48	0·50	γ Urs. M. and β Corvi	0	+ 0·35	Transit swagged to-night
Ditto	,,	,,	— 0·31	0·48	Ditto	0	+ 0·35	Transit again wavered.
78 Geminorum *N. A.* β	7 36 07·42	61 37	+ 0·26	0·42	Cast.² and ι Can. M.	1	— 0·19	Level W. + 0″·49.
Ditto	,,	,,	+ 0·24	0·24	Mean of Grh stars	1	— 0·11	Good short series.
Ditto	,,	,,	+ 0·69	W. ·20	α Urs. M. and Reg.	0	+ 0·09	Very few transits.
Ditto	,,	,,	+ 0·31	0·20	γ Urs. M. and β Corvi	0	— 0·09	Pollux as usual differs from the rest each night.
Ditto	,,	,,	+ 0·34	0·45	Ditto	1	— 0·20	A good night's work.
Ditto	,,	,,	+ 0·48	0·81	ι Urs. M. and α Hydr.	0	— 0·37	The clock error by this star too small with a slow clock.
Ditto	,,	,,	+ 0·30	1·00	η Urs. M. and Spica	1	+ 0·45	The clock error by this star too large with a fast clock.
Ditto		,,	+ 0·12	0·47	Castor² and β Corvi	2	+ 0·22	Compared with the orthodox reduction.
Ditto	,,	,,	+ 0·30	0·48	γ Urs. M. and β Corvi	1	+ 0·22	Transits very regular; also compared as above.

OBSERVATIONS.

Date.	Star's name.		Telescope wires. I.	II.	Centre.	IV.	V.	Mean Transit.	Clock's Error.	Clock's Daily rate.	Remarks.
			secs.	secs.	h. m. s.	secs.	secs.	s.	s.	s.	
1839 25 Feb.	19 Cancri *comp.*	λ	0·7	21·0	8 10 41·4	1·7	22·0	41·36	— 17·17	— 0·87	Rate with δ Gem. yesterday. The discrepancy of this rate with the following should be examined.
„	43 Cancri *comp.*	γ	1·2	21·3	8 33 41·5	1·7	21·8	41·50	— 18·17	— 1·57	Rate with δ Gem yesterday.
20 April	47 Cancri *comp.*	δ	21·7	41·2	8 35 0·8	20·5	40·0	0·84	— 32·10	— 1·05	Rate with δ Leon yesterday. Level E. + 0''·42.
21 „	Ditto		19·9	39·4	8 34 59·1	18·7	38·3	59·08	— 33·91	— 1·75	Rate with itself yesterday. Cloudy.
1838 21 April	11 Hydræ *N. A.*	ε	3·8	22·5	8 37 41·0	59·7	18·2	41·04	— 31·55	— 0·58	Feared moon parallax. Level W. 1''·47. Rate with Reg. 2 days previous.
1839 25 March	77 Cancri *comp.*	ξ	34·0	54·2	9 0 14·5	34·8	54·9	14·48	+ 7·03	— 1·58	Rate with Reg. 3 days after. Returned axis to its usual position, illuminated end W.
20 April	Ditto		54·7	14·9	8 59 34·9	55·0	15·1	34·92	— 32·18	— 1·67	Very steady. Rate with δ Leonis yesterday.
21 „	Ditto		53·0	13·0	8 59 33·0	53·0	13·3	33·06	— 34·02	1·84	Rate with itself yesterday
1838 21 April	22 Hydræ *comp.*	θ	48·6	7·0	9 05 25·7	44·0	2·8	25·62	— 31·55	— 0·58	Rate with Reg. 2 days previous. Level W + 1''·47.
1839 6 April	5 Cephei *N.A.* S.P.	α	14·0	53·5	9 14 32·8	12·5	51·5	32·86	— 8·97	— 1·42	Rate with γ Ceph. 10 days previous.
24 April	30 Hydræ *N.A.*	α	24·0	42·9	9 20 01·8	20·3	30·0	1·60	+ 19·77	— 1·07	Steady. Put clock forward. Rate by α Urs. next day
25 Feb.	4 Leonis *comp.*	λ	35·0	55·2	9 22 15·6	35·8	56·0	15·52	— 18·17	1·57	Rate by δ Gem. previous day.
21 April	Ditto		18·7	39·0	9 21 59·3	19·7	39·9	59·32	— 33·92	— 1·74	Steady. Rate with ξ Cancri yesterday.
25 Feb.	14 Leonis *comp.*	ο	38·8	57·9	9 32 16·8	35·8	54·0	16·84	— 18·40	1·67	Rate with Reg. 2 days after.
21 April	16 Leonis *comp.*	ψ	46·5	5·7	9 34 24·8	44·0	3·2	24·84	— 31·02	— 1·83	Rate by δ Canc. yesterday. Hazy.
28 March	17 Leonis *N.A.*	ε	6·4	26·9	9 36 47·3	7·6	28·0	47·24	+ 2·91	— 1·36	Cloudy, star faint. Rate with ζ Cancri 3 days previous.
6 April	Ditto		53·7	14·0	9 36 34·4	54·8	15·0	34·38	— 9·82	— 1·51	Level E. + 0''·51. Rate with Sirius next day.
10 „	Ditto		47·4	7·8	9 36 28·0	48·4	8·9	28·10	— 16·05	— 1·56	Rate with itself 4 days previous.
28 March	24 Leonis *comp.*	μ	59·2	20·0	9 43 40·8	1·5	22·2	40·74	+ 3·38	— 0·32	Unsteady. This error exceeds that of ε Leonis by 0·44, though very near in P.D.
28 March	29 Leonis *comp.*	π	8·8	27·5	9 51 46·2	5·0	23·8	46·26	+ 2·45	— 1·33	Unsteady. Rate with π Leon. yesterday.
25 March	30 Leonis *comp.*	η	3·0	22·5	9 58 42·0	1·5	21·0	42·00	+ 7·00	— 1·65	Extremely faint. Rate with ρ Leon. 2 days after.

DEDUCTIONS.

Star's name.	AR. 1 Jan. 1860.	Approx. P.D.	Δ Ast. C.	Daily Azim. Dev. E.	Stars compared for daily Azim.	Wt.	Az. correction.	Remarks.
	h. m. s.	° ′	s.	s.			s.	
19 Cancri . . . λ	8 11 36·21	65 30	+ 0·90	0·50	η Drac. S.P. and γ Erid.	0	— 0·25	Unfavourable P.D. for the instrument.
43 Cancri . . . γ	8 34 35·70	68 00	+ 0·23	0·50	Ditto	1	— 0·27	N. merid. mark exactly central.
47 Cancri . . . δ	8 36 08·95	71 18	+ 0·39	0·81	ι Urs. M. and α Hydr	1	— 0·46	Very steady.
Ditto	„	„	+ 0·47	0·53	Ditto	0	— 0·31	Steady, but some stars wild.
11 Hydræ . . . ε	8 38 49·76	83 02	— 0·46	0·41	Aldeb. and β Corvi	0	— 0·29	Transits begin to be regular.
77 Cancri . . . ξ	9 00 43·51	67 21	+ 0·26	0·62	Capel. and Rigel	2	+ 0·33	Level W. + 0″·56.
Ditto	„	„	+ 0·38	0·81	ι Urs. M. and α Hydr.	1	— 0·42	Very steady.
Ditto	„	„	+ 0·29	0·53	Ditto	2	— 0·27	In P.D. 61° to 68° clock error, too small when— and too large when +.
22 Hydræ . . . θ	9 06 33·14	87 03	— 0·42	0·41	Aldeb. and β Corvi	0	— 0·36	An initiatory night.
5 Cephei S.P. . . . α	9 14 58·26	S.P. 28 03	+ 1·17	W. ·20	α Ursæ M. and Regulus	0	+ 0·39	A puzzling night.
30 Hydræ . . . α	9 20 12·65	98 00	— 0·11	0·64	Capel and Rigel	2	+ 0·55	Steady. Level. W. 0″·11.
4 Leonis . . . . λ	9 23 09·23	66 23	+ 0·28	0·50	η Drac. S.P. and γ Erid.	2	— 0·25	N. merid. mark exact.
Ditto	„	„	+ 0·52	0·53	ι Urs. M. and α Hydr.	1	— 0·27	Steady.
14 Leonis . . . . ο	9 33 08·12	79 26	+ 0·07	0·50	η Drac. S.P. and γ Erid.	3	— 0·33	A moon culminator.
16 Leonis . . . . ψ	9 35 33·27	75 18	+ 0·49	0·53	ι Urs. M. and α Hydr.	1	— 0·33	An interesting Lunar series.
17 Leonis . . . . ε	9 37 19·40	65 32	+ 0·55	0·47	μ Urs. M. and Reg.	0	+ 0·24	A stiff night.
Ditto	„	„	+ 0·64	W. ·20	ι Urs. M. and Reg.	1	+ 0·10	A puzzling night.
Ditto	„	„	+ 0·26	0	θ Urs. M. and Sirius	1	0	No satisfactory az. dev.
24 Leonis . . . . μ	9 44 13·08	63 17	+ 0·98	0·47	μ Urs. M. and Reg.	0	+ 0·22	Re examined in vain.
29 Leonis . . . . π	9 52 16·90	81 14	+ 0·16	0·47	Ditto	3	+ 0·32	A full night.
30 Leonis . . . . η	9 59 08·61	72 30	+ 0·32	0·62	Capel. and Rigel	1	+ 0·32	Few transits.

OBSERVATIONS.

Date.	Star's name.	Telescope wires. I.	II.	Centre.	IV.	V.	Mean Transit.	Clock's Error.	Clock's Daily rate.	Remarks.
		secs.	secs.	h. m. s.	secs.	secs.	s.	s.	s.	
1839 10 April	32 Leonis *N. A.* . α	55·0	14·0	9 59 33·0	52·0	11·2	33·04	—16·53	—1·52	Rate with Reg. 4 days previous.
19 "	Ditto	40·0	59·0	9 59 18·0	37·0	56·4	18·08	—31·39	—1·65	Rate with itself 9 days previous.
24 "	Ditto	31·0	50·0	10 0 9·2	28·5	47·8	9·30	+10·89	—1·23	Rate with itself next day. Tremulous. Level W. + 0″·11. Clock put forward.
15 July	Ditto	07·9	26·7	9 59 15·5	04·5	23·7	45·66	— 3·09	— 0·04	Rate with Aldeb. 3 days previous. The clock pendulum was raised 3 divisions on 17th June, which corrected its rate.
27 Feb.	33 Ursæ Majoris *comp.* λ	12·0	37·8	10 07 03·5	29·2	55·0	3·50	—20·82	—1·36	AR. from Cat. of 1112 stars. Rate with Castor 3 days previous.
28 March	Ditto	36·0	01·6	10 07 27·2	53·0	18·7	27·30	+ 2·77	—1·48	Tolerably good. Rate with Capel. 3 days previous.
1 April	Ditto	29·4	55·0	10 07 20·9	46·6	12·4	20·88	— 3·60	—1·57	Rate with itself 4 days previous.
6 "	Ditto	23·0	48·6	10 07 14·5	40·0	05·7	14·36	—10·06	—1·29	Rate with itself 5 days previous.
10 "	Ditto	16·5	42·3	10 07 08·0	33·8	59·5	8·02	—16·34	—1·57	Rate with itself 4 days previous.
27 Feb.	41 Leonis *comp.* . γ	06·8	26·7	10 10 46·5	06·2	26·2	46·48	—20·81	—1·40	Rate with δ Gem. 3 days previous.
1 April	34 Ursæ Majoris *comp.* μ	51·8	16·9	10 12 42·0	07·2	32·2	42·02	— 3·56	—1·51	Rate with itself 4 days previous.
6 "	Ditto	45·0	10·0	10 12 35·2	00·4	25·6	35·24	—10·28	—1·68	Rate with itself 5 days previous.
10 "	Ditto	39·0	04·0	10 12 29·0	54·2	19·4	29·12	—16·38	—1·52	Rate with itself 4 days previous.
27 Feb.	42 Hydræ *comp.* . μ	19·8	39·0	10 17 58·4	17·0	37·2	58·46	—21·93	—1·88	Rate comp. with γ′ Erid. 2 days previous.
27 Mar.	47 Leonis *comp.* . ρ	. .	6·6	10 24 25·6	44·4	3·2	25·49	+ 03·70	—1·05	Flying clouds. Rate with η Leo. 2 days previous.
27 Feb.	53 Leonis *comp.* . l	50·0	9·0	10 40 28·0	47·0	6·0	28·00	—21·23	—1·54	Rate with Aldeb. 2 days previous.
1838 8 May	50 Urs. Majoris *N. A.* α	37·8	18·3	10 53 58·7	39·0	19·1	58·58	+ 15·06	—0·73	W. End level + 0″·08. Rate with itself 2 days after.
1849 5 May	Ditto	35·5	15·8	10 53 56·2	36·5	16·9	56·18	+ 08·26	—1·16	Rate with α Drac. 6 days after.
1838 3 May	63 Leonis *comp.* . χ	23·1	42·2	10 56 01·0	20·0	38·8	1·02	—39·88	—1·17	Rate with γ Virg. 2 days after. No error of collimation.
1839 27 Feb.	Ditto	45·4	4·2	10 56 23·0	41·9	0·7	23·04	— 21·45	—1·40	Rate with Procyon 3 days previous.
24 April	Ditto	27·0	45·8	10 57 04·4	23·0	41·8	4·40	+ 19·99	—1·06	Rate with α Orion. 2 days after. Level W. + 0″·11. Weather clear, but stars tremulous.
5 May	Ditto	15·0	34·0	10 56 52·5	11·0	30·0	52·50	+ 08·69	—0·77	Rate with Procyon 3 days previous.

DEDUCTIONS.

Star's name.	AR. 1 Jan. 1850.	Approx. P.D.	Δ Ast. C.	Daily Azim. Dev. E.	Stars compared for daily Azim.	Wt.	Az. correction.	Remarks.
	h. m. s.	° ′	s.	s.			s.	
32 Leonis . . . a	10 00 22·71	77 18	— 0·19	0	θ Ursæ Maj. and Sirius	0	0	This transit must be wrong.
Ditto	„	„	— 0·37	0·45	γ Urs. M. and β Corvi	0	— 0·29	
Ditto	„	„	— 0·09	0·64	Capella and Rigel	1	+ 0·42	A long series.
Ditto	„	„	+ 0·07	0·49	γ Drac. and μ Sagitt.	3	— 0·31	Steady night.
33 Ursæ Majoris . λ	10 08 02·06	46 20	+ 0·67	0·16	a Ursæ Maj. and Reg.	0	+ 0·33	The Astr. Soc. Cat. differs by 0s·20 from that of 1112 stars.
Ditto	„	„	+ 0·26	0·47	μ Ursæ Maj. and Reg.	3	+ 0·09	Five unknown stars to-night.
Ditto	„	„	— 0·02	0·49	γ Ursæ Maj. and Reg.	3	— 0·10	Pretty good.
Ditto	„	„	+ 0·49	W. 0·20	a Ursæ Maj. and Reg.	0	+ 0·04	Or Az. dev. E. 1″·10.
Ditto	„	„	+ 0·01	E. 0	θ Ursæ Maj. and Sirius	3	0	Unsatisfactory night.
41 Leonis, *comp.* . γ	10 11 41·01	69 24	+ 0·94	0·16	a Ursæ Maj. and Reg.	0	+ 0.09	Greenwich stars too close.
34 Ursæ Majoris . μ	10 13 22·59	47 45	+ 0·03	0·49	γ Ursæ Maj. and Reg.	1	— 0·11	Epps computed a pivot equation.
Ditto	„	„	+ 0·29	W. 0·20	a Ursæ Maj. and Reg.	2	+ 0·04	Few transits and irregular.
Ditto	„	„	— 0·03	E. 0		3	0	Irregular transits.
42 Hydræ. . . . μ	10 18 49·91	106 04	+ 0·45	0·16	a Ursæ Maj. and Reg.	1	+ 0·15	Greenwich stars too close.
47 Leonis . . . . ρ	10 24 54·46	79 49	— 0·13	0·45	By estimation.	0	+ 0·31	No good Greenwich stars.
53 Leonis l	10 41 21·95	78 40	+ 0·50	0·16	a Ursæ Maj. and Reg.	1	+ 0·11	Transits far north not safe.
50 Ursæ Majoris . a	10 54 25·98	27 27	— 0·31	0·61	η Ursæ Maj. and Spica	6	— 0·24	Returned axis to illumined end W.
Ditto	„	„	— 0·48	0·48	γ Urs. M. and β Corvi	0	— 0·20	Compared with orthodox reduction, the only discordant observation.
63 Leonis . . . . χ	10 57 15·97*	81 51	+ 0·42	2·20	γ Cep. S.P. and Spica	0	+ 0·11	No sensible collimation.
Ditto	„	„	+ 0·28	0·16	a Ursæ Maj. and Reg.	3	+ 0·11	Irregular night.
Ditto	„	„	+ 0·07	0·64	Cap. and Rigel.	2	+ 0·45	Tough night.
Ditto	„	„	+ 0·49	0·48	γ Urs. M. and β Corvi	3	+ 0·34	Agrees with the orthodox.

OBSERVATIONS.

Date.	Star's name.	Telescope wires. I.	II.	Centre.	IV.	V.	Mean Transit.	Clock's Error.	Daily rate.	Remarks.
		secs.	secs.	h. m. s.	secs.	secs.	s.	s.	s.	
1838 6 May	68 Leonis, *N. A.* . ϑ	8·5	28·1	11 04 48·0	8·5	28·0	48·22	—42·41	—0·84	Rate with *κ* Leo 3 days previous. Level W. + 0″·13, illum. end W.
8 „	Ditto	6·4	26·0	11 05 46·0	6·0	26·0	46·08	+ 15·47	—0·54	Rate with Arct. the previous day. Level W. end + 0″·37.
7 June	Ditto	. .	. .	11 05 02·7	22·8	42·8	2·75	—27·54	—1·30	Rate with Arct. 8 days previous. Object glass defective.
1839 19 April	Ditto	24·0	43·8	11 05 03·8	23·8	43·8	3·84	— 30·51	—1·53	Steady. Rate with Pollux 3 days previous. Level E. + 1″·25.
24 April	Ditto	14·9	34·7	11 05 54·6	14·4	34·4	54·60	+ 20·29	—1·08	Rate with Poll. 2 days previous.
5 May	Ditto	2·6	22·8	11 05 42·8	2·8	22·9	42·78	+ 08·57	—1·10	Rate with itself 2 days previous.
1838 6 May	12 Hydræ et Crateris *N. A.* . . . . . ϑ	54·8	14·0	11 10 32·9	52·0	11·0	32·94	—43·16	—1·04	Rate with Spica 3 days previous.
8 May	Ditto	52·8	11·9	11 11 30·9	50·0	9·5	31·02	+ 14·95	— 1·06	Rate with Arct. yesterday.
1839 19 April	Ditto	10·0	29·0	11 10 48·0	7·3	26·6	48·18	— 31·39	—1·51	Lev. E. + 0″·42. Rate with *β* Corvi 2 days previous.
24 „	Ditto	1·0	19·9	11 11 39·0	58·0	17·2	39·02	+ 19·48	—1·05	Rate with itself 11 days after.
5 May	Ditto	49·2	8·2	11 11 27·2	46·4	5·6	27·32	+ 7·88	—1·05	Rate with itself 11 days before.
1838 3 May	77 Leonis *comp.* . . *σ*	31·0	49·7	11 12 08·5	27·0	46·0	8·44	—40·05	—1·08	Rate with ϑ Hyd. Cr. 3 days sub.
1839 27 Mar.	Ditto	18·2	37·0	11 12 55·7	14·8	33·0	55·64	+ 3·78	—1·42	Rate with itself next day.
28 Mar.	Ditto	16·8	35·5	11 12 54·2	13·0	31·6	54·22	+ 2·36	—1·42	Rate with itself yesterday.
1838 21 April	15 Hydræ et Crateris *comp.* . . . . . . *γ*	39·2	57·9	11 16 17·5	36·8	56·3	17·54	—31·40	—0·78	Rate with ϑ Hyd. et Cr. 15 days sub. Level W. + 1″·84.
1839 24 April	84 Leonis . . . . *τ*	23·7	42·3	11 20 01·0	19·6	38·2	0·96	—19·51	— 1·06	Rate with ϑ Hyd. et Cr. 11 days after.
1838 21 April	21 Hydræ et Crateris *comp.* . . . . . . *θ*	32·4	50·4	11 28 09·5	28·0	46·5	9·36	— 31·27	—0·73	Rate with Spica 12 days after. Level W. + 1″·84.
1839 28 Mar.	91 Leonis *comp.* . . *υ*	9·3	27·9	11 28 46·5	5·1	23·8	46·52	+ 2·24	—1·54	Rate with *σ* Leo. yesterday N.A. makes clk. + 0·85 only.
5 May	35 Cephei *N.A.* S.P. *γ*	10·0	31·0	11 32 52·0	13·0	33·8	51·96	+ 8·93	—1·33	Rate with itself 2 days before.
1838 21 April	94 Leonis *N. A.* . *β*	40·0	59·0	11 40 18·5	37·5	56·7	18·34	—31·07	—0·77	Rate with ϑ Leo. 5 days previous. Level W. + 1″·84.
7 June	Ditto	42·4	1·8	11 40 21·1	40·5	0·1	21·18	— 27·84	—1·47	Rate with Arct. 2 days after.
1839 17 April	Ditto	46·0	5·5	11 40 25·0	44·0	3·3	24·76	— 28·09	—1·65	Rate with Reg. 7 days previous.
19 April	Ditto	43·0	2·5	11 40 21·8	41·0	0·2	21·70	— 31·14	—1·53	Level E. + 0″·42. Rate with itself 2 days previous.

DEDUCTIONS.

Star's name.	AR. 1 Jan. 1850.	Approx. P.D.	Δ Ast. C.	Daily Azm. Dev. E.	Stars compared for daily Azm.	Wt.	Az. correction.	Remarks.
	h. m. s.	° ′	s.	s.			s.	
68 Leonis . . . . δ	11 06 06·97	68 39	+ 0·32	0·66	Capella and Sirius	0	— 0·36	Axis, illuminating end W.
Ditto	”	”	+ 0·47	0·61	η Urs. Maj. and a^2 Lib	1	+ 0·33	Again reversed axis.
Ditto	”	”	+ 0·31	0·87	η Ursæ Maj. and Spica	0	— 0·48	A good night's work.
Ditto	”	”	+ 0·54	0·45	γ Urs. M. and β Corvi	1	— 0·25	Stars north of zenith no help.
Ditto	”	”	+ 0·29	0·64	Capella and Rigel	1	+ 0·39	A good night's work.
Ditto	”	”	+ 0·30	0·48	γ Urs M. and β Corvi	2	+ 0·26	Agrees with orthodox reduction.
12 Hydræ and Crateris δ	11 11 50·60	103 58	— 0·18	0·66	Capella and Sirius	0	— 0·62	The only unknown star was γ Virg.
Ditto	”	”	+ 0·10	0·61	η Urs. M. and a^2 Lib.	0	+ 0·58	North stars unsafe.
Ditto	”	”	— 0·16	0·45	γ Urs. M. and β Corvi	3	— 0·42	Stars north of zenith no aid.
Ditto	”	”	— 0·26	0·64	Capella and Rigel	1	+ 0·62	By several comparisons still —.
Ditto	”	”	— 0·19	0·48	γ Ursæ M. and β Corvi	3	+ 0·46	Agrees with orthodox reduction.
77 Leonis . . . . σ	11 13 23·19	83 09	+ 0·51	2·30	λ Draco and Spica	0	— 0·67	Not recommended.
Ditto	”	”	+ 0·22	0·45	Brought from 25th ins.	2	+ 0·32	No good Greenwich stars.
Ditto	”	”	+ 0·13	0·47	μ Ur. M. and α Leo.	3	+ 0·33	A valuable night.
15 Hydræ and Crateris γ	11 17 23·12	106 51	+ 0·16	0·41	Aldeb. and β Corvi	3	— 0·40	First night reduced.
84 Leonis . . . . τ	11 20 12·09	86 19	+ 0·37	0·64	Capella and Rigel	2	+ 0·47	Still — by three comparisons.
21 Hydræ and Crateris θ	11 29 04·19	98 58	+ 0·05	0·41	Aldeb. and β Corvi	2	— 0·36	A trial night.
91 Leonis . . . . υ	11 29 15·87	90 00	+ 0·26	0·47	μ Urs. M. and α Leo	3	— 0·37	(Epps) Differs by 0·16 — in clock-error.
35 Cephei S.P. . . γ	11 33 12·60	S. P. 13 13	+ 1·04	0·48	γ Urs. M. and β Corvi	2	+ 1·63	Orthodox correction still more.
94 Leonis . . . β	11 41 24·05	74 35	+ 0·08	0·41	Aldeb. and β Corvi	0	— 0·26	Initiatory night.
Ditto	”	”	+ 0·10	0·87	η Urs. M. and Spica	2	— 0.54	Errors large.
Ditto	”	”	+ 0·02	0·50	γ Urs. M. β Corvi	2	— 0·31	Few, but good.
Ditto	”	”	+ 0·03	0·45	Ditto	3	0·28	Fine night.

OBSERVATIONS.

Date.	Star's name.	Telescope wires. I.	II.	Centre.	IV.	V.	Mean Transit.	Clock's Error.	Daily rate.	Remarks.
		secs.	secs.	h. m. s.	secs.	secs.	s.	s.	s.	
1839 2 May	94 Leonis *N.A.* . β	25·8	45·0	11 41 04·0	23·1	42·5	4·08	+ 11·32	— 0·92	Rate with itself 3 days after.
5 May	Ditto	22·0	42·0	11 41 01·9	20·8	39·8	1·30	+ 08·56	— 0·92	Rate with itself 3 days be- before.
24 April	5 Virginis *comp.* . β	3·2	21·9	11 42 40·4	59·0	17·7	40·44	+ 19·64	— 1·34	Rate with Sir. 2 days previous. Level W. + 0″·11.
16 April	64 Ursæ Majoris *N.A.* γ	53·0	25·0	11 44 57·0	29·0	1·0	57·00	— 26·46	— 1·31	Rate with itself next day.
19 April	Ditto	48·5	20·8	11 44 52·6	24·5	56·7	52·62	— 30·80	— 1·11	Rate with α Cass. 2 days previous.
24 April	Ditto	39·5	11·4	11 45 43·5	15·8	47·9	43·02	+ 20·27	— 1·18	Rate with a mean of 2 days.
1838 24 Nov.	Ditto S.P.	27·5	59·7	23 45 32·0	4·0	35·9	31·82	+ 12·50	— 1·16	Rate with itself 2 days after.
1838 21 April	8 Virginis *comp.* . π	27·2	45·7	11 52 04·3	23·5	42·0	4·54	— 31·43	— 0·70	Rate with λ Leonis 12 days after. Level W. + 1″·84.
1839 2 May	Ditto	13·0	31·7	11 52 50·2	8·9	27·8	50·32	+ 10·98	— 0·98	Rate with Procyon 3 days after.
23 March	9 Virginis *comp.* . ο	27·0	46·0	11 57 05·0	23·9	42·7	4·92	+ 02·24	— 1·46	Very steady. Rate with ε Leo yesterday.
24 April	Ditto	44·8	3·7	11 57 22·6	41·5	0·4	22·60	+ 19·94	— 1·20	Rate with η Virg. 3 days after.
2 May	69 Ursæ Majoris *comp.* δ	30·8	5·8	12 07 40·7	15·8	51·0	40·82	+ 12·38	— 0·93	Rate with itself 3 days after.
5 May	Ditto	27·8	2·7	12 07 37·8	12·8	47·8	37·78	+ 09·48	— 0·98	Rate with γ Ursæ Maj. 11 days previous.
28 March	15 Virginis *comp.* . η	7·2	25·8	12 11 44·4	3·0	21·5	44·38	+ 02·17	— 1·44	Good. Rate with α Orion. 4 days previous
19 April	Ditto	34·0	52·5	12 11 11·1	29·6	48·2	11·08	— 31·25	— 1·51	Rate with ι Virg. 2 days before.
27 April	Ditto	21·5	39·9	12 11 58·5	16·9	35·6	58·48	+ 16·17	— 1·26	Rate with ο Virg. 3 days before.
19 April	9 Corvi *N.A.* . β	47·0	7·1	12 25 57·2	47·2	7·5	27·20	— 31·35	— 1·55	Rate with itself 3 days previous.
24 April	Ditto	38·0	58·0	12 26 18·0	38·1	58·2	18·06	+ 10·52	— 1·40	Rate with Sirius 2 days previous.
1838 6 May	29 Virginis *comp.* . γ	9·0	27·5	12 32 46·0	4·5	23·0	46·00	— 42·00	— 0·87	Rate with itself 12 days previous.
1839 19 April	Ditto	24·0	42·5	12 33 01·0	19·5	38·0	1·00	— 31·35	— 1·55	Rate with β Corvi 3 days previous.
2 May	Ditto	6·3	25·0	12 33 43·5	2·0	20·7	43·50	+ 11·40	— 1·08	Rate with itself 3 days after.
5 May	Ditto	3·0	21·7	12 33 40·3	58·8	17·5	40·26	+ 08·17	— 1·09	Rate with Sirius 2 days previous.
27 April	12 Canum Venatici *A.S.C.* . . . . . α	. .	. .	12 48 48·8	12·7	36·8	48·80	+ 16·88	— 1·21	This clock error agrees with the rest. Rate with Capella 3 days previous.
”	Ditto *N.A.*	. .	. .	. . . .	. .	. .	. .	+ 16·41	— 1·30	Only three wires observed.
2 May	Ditto *N.A.*	55·8	19·8	12 48 43·7	7·7	31·6	43·72	+ 11·35	— 1·67	Rate with itself 3 days previous.

DEDUCTIONS.

Star's name.		AR. 1 Jan. 1850.	Approx. P.D.	Δ Ast. C.	Daily Azim. Dev. E.	Stars compared for daily Azim.	Wt.	Az. correction.	Remarks.
		h. m. s.	° ′	s.	s.			s.	
94 Leonis . . .	β	11 41 24·05	74 35	− 0·03	0·50	γ Urs. M. and β Corvi	1	+ 0·31	Instrument swagged.
Ditto		”	”	+ 0·35	0·48	Ditto	3	+ 0·30	Agrees with orthodox reduction.
5 Virginis . . .	β	11 42 52·66*	87 24	− 0·21	0·64	Capella and Rigel	1	+ 0·48	Troublesome night.
64 Ursæ Majoris .	γ	11 45 54·67	35 29	− 0·03	0·20	γ Ceph. S.P. and β Corvi.	2	+ 0·01	Tried several pairs of stars
Ditto		”	”	+ 0·04	0·45	γ Urs. M. and β Corvi	1	+ 0·04	Star of comparison.
Ditto		”	”	− 0·12	0·64	Capella and Rigel	0	− 0·06	Near the zenith.
Ditto		”	”	+ 0·03	0·12	γ Ceph. and Fomal.	1	+ 0·01	Tried several pairs of stars.
8 Virginis . . .	π	11 53 11·09	82 34	− 0·25	0·41	Aldeb. and β Corvi	2	− 0·28	All low stars except Polaris.
Ditto		”	”	− 0·32	0·50	γ Urs. M. and β Corvi	0	+ 0·35	Clock-error a crooked line.
9 Virginis . . .	o	11 57 34·13*	80 27	+ 0·06	0·47	μ Urs. M. and α Leo.	2	+ 0·31	An interesting night.
Ditto		”	”	+ 0·05	0·64	Capella and Rigel.	0	+ 0·43	A long regular night.
69 Ursæ Majoris .	δ	12 07 57·64	32 09	+ 0·65	0·50	γ Urs. M. and β Corvi	1	+ 0·10	Agrees with orthodox reduction.
Ditto		”	”	+ 0·88	0·48	Ditto	2	− 0·10	Agrees with orthodox reduction.
15 Virginis . . .	η	12 12 12·90	89 50	− 0·09	0·47	μ Urs. M. and α Leo.	3	+ 0·36	A moon culminator.
Ditto		”	”	− 0·02	0·45	γ Urs. M. and β Corvi.	3	− 0·36	Long night.
Ditto		”	”	+ 0·28	0·81	η Boot. and β Corvi	1	+ 0·62	Agrees nearly with orthodox reduction.
9 Corvi	β	12 26 30·37	112 34	− 0	0·45	γ Urs. M. and β Corvi	0	− 0·47	Star comparable to Spica.
Ditto		”	”	− 0·25	0·64	Capella and Rigel	1	+ 0·66	Good night's work.
29 Virginis . . .	γ	12 34 03·23	90 38	+ 0·04	0·06	Capella and Sirius	0	− 0·58	A short initiatory night.
Ditto		”	”	− 0·11	0·45	γ Urs. M. and β Corvi	1	− 0·36	Still—by another comparison.
Ditto		”	”	+ 0·18	0·50	η Urs. M. and β Corvi	. .	+ 0·40	Compared with orthodox reduction.
Ditto		”	”	+ 0·08	0·48	Ditto	0	+ 0·39	Correct. of AR by orthodox reduct.—0·11.
12 Canum Venatic.	α	12 49 00·25	50 52	+ 0·08	0·81	η Boot. and β Corvi	2	+ 0·24	Polaris gives error of AR. —0·09.
Ditto		”	”	− 0·39	0·81	Ditto	0	+ 0·24	This shows that the NA. elements do not suit this star.
Ditto		”	”	− 0·11	0·50	γ Urs. M. and β Corvi	2	+ 0·14	Compared with orthodox reduction.

OBSERVATIONS.

Date.	Star's name.	Telescope wires. I.	II.	Centre.	IV.	V.	Mean Transit.	Clock's Error.	Clock's Daily rate.	Remarks.
		secs.	secs.	h. m. s.	secs.	secs.	s.	s.	s.	
1839 16 April	47 Virginis *comp.* . ε	7·5	26·4	12 58 45·8	4·2	23·1	45·30	−26·93	−1·30	Rate with itself next day.
,,	Ditto	. .	. .	. . .	. .	. .	. . .	−26·81	. . .	By A.S.C. only.
17 ,,	Ditto	6·0	25·0	12 53 44·0	3·0	22·0	44·00	−28·23	−1·30	Good coincidence. Rate with itself previous day.
19 April	Ditto	. .	21·7	12 53 40·9	59·9	18·8	40·84	−31·27	−1·52	Rate with itself 2 days previous.
27 ,,	Ditto	50·7	9·8	12 54 28·8	47·7	6·7	28·74	+ 16·62	−10·9	Rate with Reg. 3 days previous.
1838 8 May	67 Virginis *N.A.* . α	18·8	37·6	13 16 56·4	15·5	34·5	56·56	+ 14·66	−1·10	Rate with itself previous day.
1839 19 April	Ditto	36·0	55·0	13 16 14·0	33·1	52·1	14·04	−31·32	−1·48	Rate with itself 2 days previous.
27 ,,	Ditto	23·6	42·7	13 17 1·7	20·6	39·3	1·58	+ 16·19	−1·04	Bad figure. Rate with itself next day.
11 May	79 Virginis *comp.* . ζ	55·7	14·1	13 26 32·7	51·2	10·0	32·74	+ 0·60	−1·20	Mean of rate.
27 April	82 Virginis *comp.* . *m*	50·8	9·6	13 33 28·2	47·0	5·8	28·28	+ 15·98	−0·92	Rate with itself 2 days after.
29 ,,	Ditto	49·0	7·7	13 33 26·4	45·2	4·0	26·46	+ 14·15	−1·06	Rate with itself 12 days after.
11 May	Ditto	35·8	54·5	13 33 13·0	31·7	50·4	13·08	+ 1·43	−1·20	Mean of rate.
1838 11 May	85 Ursæ Majoris *N.A.* η	26·4	55·2	13 41 24·2	53·3	22·0	24·22	+ 12·84	−1·21	Rate with itself next day.
30 Aug.	Ditto	24·1	53·0	13 41 21·0	51·0	20·0	22·00	+ 12·25	−2·04	Very steady. Rate of β Drac.
7 May	8 Bootis *N.A.* . . . η	37·4	57·0	13 47 16·8	36·5	56·0	16·74	+ 16·13	−1·00	Rate with itself 4 days after.
11 May	Ditto	33·4	53·0	13 47 12·8	32·5	52·0	12·74	+ 12·12	−0·83	Rate with δ Leo. 2 days after.
8 June	Ditto	50·5	10·5	13 46 30·5	50·5	10·5	30·50	−30·53	−1·70	Steady. Rate with α Serp. previous day.
1839 29 April	Ditto	38·6	58·2	13 47 17·9	37·5	57·2	17·88	+ 14·11	−1·12	Rate with itself previous day
11 May	Ditto	25·6	45·3	13 47 05·0	24·8	44·7	5·08	+ 1·28	−1·07	Rate with itself 12 days previous.
18 July	Ditto	20·6	40·2	13 47 0·0	20·0	39·8	0·12	− 8·22	−0·10	Clock rate uncommonly steady.
1838 7 May	93 Virginis *comp.* . . τ	5·0	23·6	13 53 42·0	1·0	19·5	42·22	+ 15·90	−0·62	Rate with Arct. next day.
31 ,,	Ditto	16·4	41·7	13 53 7·0	32·5	57·9	7·10	−19·28	−1·44	Steady, good. Rate by itself 24 days previous.
2 June	Ditto	13·5	39·0	13 53 4·5	30·0	55·5	4·50	−21·85	−1·28	Rate by itself 2 days previous.
4 ,,	Ditto	24·4	42·8	13 53 1·5	20·2	39·0	1·50	−24·84	−1·50	Rate by itself 2 days previous.
8 ,,	Ditto	19·0	37·8	13 52 56·3	15·0	33·7	56·36	−30·06	−1·30	App. AR. comp. for 1112 Cat. Rate with itself 4 days previous.
1839 11 May	Ditto	53·4	12·0	13 53 30·6	49·1	7·8	30·58	+ 0·65	−1·23	Rate with Proc. 6 days previous
1838 31 May	11 Draconis *comp.* . . α	. .	45·7	13 59 45·8	46·0	46·5	45·80	−17·79	−0·66	Observed uneasily. Rate small just now.

DEDUCTIONS.

Star's name.	AR. 1 Jan. 1850.	Approx. P.D.	Δ Ast. C.	Daily Azim. Dev. E.	Stars compared for daily Azim.	Wt.	Az. correction.	Remarks.
	h. m. s.	° ′	s.	s.			s.	
47 Virginis . . . . ι	12 54 42·83	78 14	−0·28	0·20	γ Ceph.S.P. and β Cor.	1	−0·13	Ap. AR. from a mean of A.S.C. and Cat. of 1112 stars.
Ditto	"	"	−0·18	0·20	Ditto	3	−0·13	By A.S.C. only.
Ditto	"	"	−0·02	0·50	γ Urs. M. and β Corvi.	3	−0·33	A regular night.
Ditto	"	"	−0·07	0·45	Ditto	1	−0·29	Tried several pairs of stars.
Ditto	"	"	+ 0·11	0·81	" Boot. and β Corvi.	3	+ 0·52	An unsteady night.
67 Virginis . . . . α	13 17 17·05	100 22	−0·05	0·61	" Urs. M. and a^2 Lib.	2	+ 0·55	Corrected for level also.
Ditto	"	"	+ 0·02	0·45	γ Urs. M. and β Corvi	3	+ 0·41	A jirk between P.D. 68° and 74°.
Ditto	"	"	−0·11	0·81	" Boot. and β Corvi	1	+ 0·72	Tried also by Polaris.
79 Virginis . . . . ζ	13 27 03·58	89 50	−0·69	0·20	" Ursæ Maj. and Spica	0	−0·69	Confirmed by orthodox method.
82 Virginis . . . . m	13 33 43·61	97 57	−0·34	0·81	" Boot. and β Corvi	1	+ 0·69	An interesting night.
Ditto	"	"	+ 0·54	1·00	" Ursæ Maj. and Spica	0	+ 0·87	One of the best nights.
Ditto	"	"	+ 0·15	0·20	Ditto	0	+ 0·15	The orthodox reduction gives—0·62.
85 Ursæ Majoris . . η	13 41 30·43*	39 57	0	0·61	γ Drac. and Spica	0	+ 0·03	Initiatory night, index star.
Ditto	"	"	+ 0·20	1·18	β Drac. and β Ophi.	1	+ 0·06	Only 5 transits to-night.
8 Bootis . . . . . η	13 47 32·36	70 51	+ 0·04	1·00	mean and Spica	1	+ 0·57	Level W. 0″·81.
Ditto	"	"	+ 0·09	0·61	" Ursæ Maj. and Spica	2	+ 0·34	Level W. 2″·35.
Ditto	"	"	+ 0·13	1·28	mean of 7th and 9th	0	−0·70	No low Greenwich star.
Ditto	"	"	+ 0·20	1·00	" Ursæ Maj. and Spica	2	+ 0·57	By Polaris did not suit so well.
Ditto	"	"	−0·05	0·20	Ditto	0	+ 0·11	Compared with orthodox reduction.
Ditto	"	"	−0·11	0·90	" Urs. M. and β′ Scorp.	0	−0·51	Clock has become steady
93 Virginis . . . . τ	13 54 00·83	87 44	+ 0·01	1·00	mean and Spica	3	+ 0·76	A troublesome night.
Ditto	"	"	− 0·22	1·00	a mean	1	−0·76	Only four transits.
Ditto	"	"	−0·13	0·50	" Ursæ Maj. and mean	2	−0·88	Only three transits.
Ditto	"	"	−0·22	1·35	" Ursæ Maj. and Spica	0	−1·01	Short night.
Ditto	"	"	−0·16	1·23	mean of 7th and 9th	3	−0·93	Level W. 0″·02.
Ditto	"	"	−0·73	0·20	" Ursæ Maj. and Spica	0	+ 0·15	Agrees nearly with orthodox reduction.
11 Draconis . . . α	14 00 19·07	24 55	−0·05	1·00	a mean	3	+ 0·56	Plus because N. of zenith.

OBSERVATIONS.

Date.	Star's name.	Telescope wires. I.	II.	Centre.	IV.	V.	Mean Transit.	Clock's Error.	Daily rate.	Remarks.
		secs.	secs.	h. m. s.	secs.	secs.	s.	s.	s.	
1839 27 April	11 Draconis *N.A.* . . α	54·3	33·7	14 0 23·0	7·2	51·2	22·88	+ 16·95	— 0·93	Rate with α Urs. M. 3 days previous.
29 „	Ditto	52·2	36·3	14 0 20·5	4·8	49·0	20·56	+ 14·64	— 1·16	Rate with itself 2 days previous.
1838 4 June	1610 Virginis P. 317 *comp.*	59·0	18·2	14 1 37·4	56·5	16·4	37·50	— 24·25	— 1·33	Rate with Spica 3 days after. The focus not good.
7 May	16 Bootis *N.A.* . . . α	55·3	15·0	14 8 34·7	54·4	14·1	34·70	+ 16·01	. .	Clock just put forward.
8 „	Ditto	54·0	14·0	14 8 34·0	53·9	14·0	33·98	+ 15·28	— 0·73	Rate with itself yesterday.
7 June	Ditto	11·5	31·3	14 7 51·1	10·8	30·6	51·06	— 27·60	— 1·43	Rate with itself since May.
8 „	Ditto	10·0	29·5	14 7 49·2	9·0	28·5	49·24	— 29·42	— 1·82	Rate with itself yesterday.
28 Sept.	Ditto	46·6	6·4	14 8 26·2	46·0	5·8	26·20	+ 8·75	— 2·19	Rate with itself 6 days previous.
1839 27 April	Ditto	59·0	18·7	14 8 38·4	58·2	18·0	38·46	+ 16·76	— 1·17	Rate with δ Leo. 3 days previous.
29 „	Ditto	56·5	16·2	14 8 36·0	55·8	15·7	36·04	+ 14·32	— 1·22	Rate with itself 2 days previous.
11 May	Ditto	43·2	3·0	14 8 23·0	43·0	2·8	23·00	+ 1·24	— 1·09	Rate with itself 12 days previous.
5 July	Ditto	39·8	59·4	14 8 19·1	39·0	58·9	19·24	— 2·26	— 0·12	Clock uncommonly steady. Rate with itself 10 days previous.
18 „	Ditto	39·0	58·5	14 8 18·4	38·0	58·0	18·38	— 2·99	— 0·07	Rate with itself 13 days previous.
1838 7 May	100 Virginis *comp.* . . λ	1·0	20·0	14 10 39·0	58·0	17·0	39·00	+ 15·52	— 1·03	Rate with itself next day.
8 „	Ditto	0	19·0	14 10 38·0	57·0	15·9	37·98	+ 14·49	— 0·91	Rate with δ Scorp. 2 days after.
1839 27 April	Ditto	5·0	24·0	14 10 43·0	2·0	21·0	43·00	+ 16·00	— 1·21	Rate with itself next day.
1838 4 June	2 Libræ *comp.*	42·0	1·0	14 14 20·0	39·0	58·0	20·00	— 24·30	— 1·36	Bad focus. Rate with β Libræ 3 days after.
1839 5 July	27 Bootis *comp.* . . . γ	48·0	12·0	14 25 36·0	0	23·8	35·96	— 2·11	— 0·11	Clock rate small, with α Cyg.
1838 7 June	36 Bootis *N. A.* . . . ε	48·0	9·0	14 37 29·8	50·7	11·8	29·86	— 27·27	— 1·05	Rate with itself 2 days before.
28 Sept.	Ditto	22·8	43·8	14 38 4·8	25·8	46·8	4·80	+ 9·09	— 0·97	Steady. Rate with γ Aquil. previous day.
1839 29 April	Ditto	32·5	53·5	14 38 14·5	35·5	56·5	14·50	+ 14·50	— 1·20	Rate with itself previous day.
5 July	Ditto	15·8	36·8	14 37 57·8	18·8	39·8	57·80	— 2·05	— 0·04	Clock steady. Rate with itself 4 days previous.
29 April	9 Libræ *N. A.* . . . α^2	36·5	55·8	14 42 15·0	34·5	53·6	15·08	+ 13·52	— 1·16	Rate with itself previous day.
18 July	Ditto	19·7	39·0	14 41 58·0	17·1	36·2	58·00	— 3·50	— 0·09	Rate with μ' Sagit. 13 days before. Clock very steady.
1838 8 May	20 Libræ *comp.* . . . γ	12·8	33·3	14 54 53·5	13·8	34·0	53·48	+ 14·77	— 0·75	This is also called γ Scorp. Rate with λ Virg. 1 day previous.
1839 29 April	Ditto	14·8	35·3	14 54 56·0	16·6	37·2	55·98	+ 13·50	— 1·03	Rate with itself previous day.
18 July	42 Bootis *comp.* . . . β	3·0	27·7	14 55 52·1	17·0	41·8	52·32	— 2·65	— 0·03	Small rate with α Cygni 14 days previous.

DEDUCTIONS.

Star's name.	Alt. 1 Jan. 1850.	Approx. P.D.	Δ Ast. C.	Daily Azim. Dev. E.	Stars compared for daily Azm.	Wt.	Az. correction.	Remarks.
	h. m. s.	° ′	s.	s.			s.	
11 Draconis . . . α	14 00 09·	24 53	— 0·49	0·81	″ Boot. and β Corvi	0	— 0·45	Not safe N. of zenith.
Ditto	″	″	— 0·40	1·00	″ Urs. Maj. and Spica	0	— 0·58	Minus because N. of zenith.
1610 Virginis P. 317	14 02 38·81	105 35	+ 0·66	1·35	″ Urs. Maj. and Spica	1	— 1·30	Very short night.
16 Bootis . . . . . α	14 08 48·91	70 02	— 0·05	1·00	Mean and Spica	3	+ 0·56	Only two unknown stars.
Ditto	″	″	+ 0·38	0·61	″ Ur. Maj. and α^2 Lib.	3	+ 0·35	Level W. 0″·37.
Ditto	″	″	+ 0·41	0·87	″ Urs. Maj. and Spica	1	— 0·49	Tolerable night.
Ditto	″	″	+ 0·27	1·23	Mean of 7th and 9th	0	— 0·69	Stars irregular.
Ditto	″	″	+ 0·29	1·30	Mean and δ Aqui.	3	+ 0·78	Level W. + 0″·64.
Ditto	″	″	+ 0·23	0·81	″ Boot. and β Corvi	3	+ 0·45	An important night.
Ditto	″	″	+ 0·42	1·00	″ Urs. Maj. and Spica	2	+ 0·56	A long series.
Ditto	″	″	— 0·07	0·20	Ditto	3	+ 0·11	Agrees nearly with the orthodox reduction.
Ditto	″	″	— 0·01	0·40	γ Drac. and α Ophiu.	1	— 0·22	Level E. + 0″·80.
Ditto	″	″	+ 0·12	0·90	″ Ur. Maj. and β′ Scorp.	2	— 0·50	Clock steady at last.
100 Virginis . . . λ	14 10 59·82	102 41	— 0·17	1·00	Mean and Spica	2	+ 0·93	A moon culminator.
Ditto	″	″	— 0·20	0·61	″ Ur. Maj. and α^2 Lib.	2	+ 0·56	A troublesome night.
Ditto	″	″	+ 0·25	0·81	″ Boot. and β Corvi	2	+ 0·73	
2 Libræ	14 15 20·86	101 01	+ 0·44	1·35	″ Urs. Maj. and Spica	3	— 1·22	Level W. + 0″·25.
27 Bootis . . . . γ	14 26 02·26	51 03	+ 0·24	0·40	γ Drac. and α Ophiu.	3	— 0·12	Few transits, but regular.
36 Bootis . . . . . ε	14 38 25·70	62 19	+ 0·62	0·87	″ Urs. Maj. and Spica	1	— 0·40	A good series.
Ditto	″	″	+ 0·55	1·30	″ Ur. Maj. and δ Aqui.	2	+ 0·60	Nearly all Greenw. stars.
Ditto	″	″	+ 0·53	1·00	″ Urs. Maj. and Spica	0	+ 0·46	A good night's work.
Ditto	″	″	+ 0·36	0·40	Ditto	2	— 0·18	A steady night.
9 Libræ . . . α^2	14 42 35·40	105 25	+ 0·08	1·00	″ Urs. Maj and Spica	3	+ 0·95	A satisfactory night.
Ditto	″	″	— 0·03	0·90	″ Ur. Maj. and β′ Scorp.	3	— 0·86	Good trans. S. of zenith.
20 Libræ . . . γ	14 55 17·95	114 41	+ 0·20	0·61	″ Ur. Maj. and α^2 Lib.	2	+ 0·65	A tolerable series.
Ditto	″	″	+ 0·15	1·00	″ Ur. Maj. and Spica	3	+ 1·07	The clock getting troublesome.
42 Bootis . . . . β	14 56 17·44	49 02	+ 0·18	0·90	″ Ur. Maj. and β′ Scorp.	2	— 0·23	Clock rate steady

Date.	Star's name.	Telescope wires. I.	II.	Centre.	IV.	V.	Mean Transit.	Clock's Error.	Daily rate.	Remarks.
		secs.	secs.	h. m. s.	secs.	secs.	s.	s.	s.	
1838 7 June	27 Libræ *N. A.* . . β	14·0	33·0	15 7 51·8	10·6	29·5	51·78	—28·48	—1·44	Rate with Spica the next day.
1839 29 April	Ditto	59·4	18·2	15 8 37·0	55·8	14·5	36·98	+ 13·44	—1·09	Rate with 20 Libræ 1 day before.
4 July	Ditto	43·0	2·0	15 8 21·0	40·9	58·9	20·96	— 2·82	—0·05	Rate with itself 3 days previous.
15 ,,	Ditto	43·0	1·7	15 8 20·5	39·0	57·8	20·40	— 3·33	—0·05	Rate with itself 11 days before.
18 ,,	Ditto	. .	1·0	15 8 20·0	39·0	. .	20·00	— 3·71	—0·13	Rate with itself 3 days previous.
1838 28 Sept.	Corona Borealis *N. A.* α	18·0	38·6	15 27 59·5	20·6	41·3	59·60	+ 8·75	—1·31	Rate with γ Aqui. previous day.
29 Oct.	Ditto	8·0	29·0	15 27 49·9	10·8	31·7	49·88	— 0·72	—1·29	Rate with γ Aqui. 3 days previous.
1839 15 July	Ditto	10·8	. .	15 27 52·0	13·0	. .	52·00	— 2·86	—0·15	Rate with itself 3 days previous.
18 ,,	Ditto	10·0	31·0	15 27 52·0	13·0	34·0	52·00	— 2·82	—0·01	Rate with itself 3 days previous.
29 April	Libræ *comp.* . . . χ	22·2	42·5	15 31 2·8	23·0	43·2	2·74	+ 13·60	—0·84	Rate with 20 Lib. previous day.
1838 7 June	Serpentis *N.A.* . . α	14·0	33·0	15 35 51·8	10·0	29·0	51·56	—28·35	—1·68	Rate with Arct. 2 days before.
1839 29 April	Ditto	59·3	18·0	15 36 36·5	55·2	14·0	36·60	+ 13·79	—1·42	Rate with ι Virg. 2 days previous.
15 July	Ditto	42·6	1·2	15 36 19·9	38·7	57·5	19·98	— 3·11	—0·05	With large power No 4. Rate with itself 3 days after.
18 ,,	Ditto	. .	1·0	15 36 19·8	38·6	. .	19·80	— 3·27	—0·05	Rate with itself 3 days previous.
29 April	7 Scorpii *comp.* . δ	25·6	45·5	15 51 5·7	25·5	45·9	5·64	+ 13·73	—1·14	Rate with λ Virg. 2 days previous.
18 July	8 Scorpii *N.A.* . β′	25·2	45·0	15 56 4·6	24·2	44·0	4·60	— 3·51	—0·06	Wind boisterous. Rate by β Lib. 3 days previous.
18 ,,	1 Ophiuchi *N.A.* . δ	17·0	35·6	16 5 54·2	12·8	31·3	54·18	— 3·40	—0·05	Rate with β Lib. 3 days previous.
1838 7 June	20 Scorpii *comp.* . σ	14·7	35·0	16 10 55·4	16·0	36·1	55·44	—28·44	—1·79	Cloudy. Rate by θ Ophi. following day.
7 June	21 Scorpii *N.A.* . α	22·0	42·8	16 19 3·6	24·3	45·0	3·54	—28·30	—1·93	Rate by θ Ophi. following day.
11 May	23 Scorpii *comp.* . τ	20·0	41·0	16 26 2·0	23·0	44·5	2·10	+ 11·18	—1·52	Rate by itself 20 days after.
7 June	Ditto	41·0	2·0	16 25 23·0	44·0	5·0	23·00	—28·33	—1·90	Rate by θ Ophi. the following day.
11 May	25 Scorpii *comp.* P. 170	. .	50·0	16 37 11·0	31·0	51·0	10·67	+ 11·14	—1·58	Rate with θ Ophi. 27 days after.
8 June	42 Ophiuchi *comp.* . θ	55·6	16·0	17 11 36·4	57·0	17·5	36·50	—30·23	—1·93	Rate by Antares previous day.
28 Sept.	23 Draconis *N.A.* . β	54·8	25·2	17 26 55·8	26·8	56·7	55·76	+ 8·47	—2·31	Rate by α Ceph. 2 days after.

DEDUCTIONS.

Star's name.	AR. 1 Jan. 1850.	Approx. P.D.	Δ Ast. C.	Daily Azim. Dev. E.	Stars compared for daily Azim.	Wt.	Az. correction.	Remarks.
	h. m. s.	° ′	s.	s.			s.	
27 Libræ . . . . β	15 08 56·35	98 49	−0·14	0·87	η Urs. Maj. and Spica	1	−0·77	Few unknown stars.
Ditto	"	"	−0·09	1·00	η Urs. Maj. and Spica	1	+ 0·88	Level nearly correct.
Ditto	"	"	−0·17	0·63	α Cyg. and $α^2$ Capr.	0	−0·55	Some stars wild to-night.
Ditto	"	"	−0·03	0·49	γ Drac. and μ Sagitt.	3	−0·43	Sure of the deviation.
Ditto	"	"	−0·30	0·90	η Ur. Maj. and β′ Scorp.	0	−0·80	Good only S. of zenith.
Coronæ Borealis . . α	15 28 19·62	62 47	+ 0·30	1·30	η Ur. Maj. and δ Aqui.	3	+ 0·61	Level W. + 0″·64.
Ditto	"	"	+ 0·51	1·28	α Cyg. and $α^2$ Capric.	1	−0·50	A good range of stars.
Ditto	"	"	+ 0·24	0·49	γ Drac. and μ Sagitt.	2	−0·23	Safe in the deviation.
Ditto	"	"	+ 0·10	0·90	η Ur. Ma. and β′ Scorp.	1	−0·42	Clock rate all but perfect.
Libræ . . . . . χ	15 31 24·06	113 19	+ 0·35	1·00	η Urs. Maj. and Spica	2	+ 1·05	Level same as yesterday, nearly correct.
Serpentis . . . . α	15 36 52·02	83 06	−0·33	0·87	η Urs. Maj. and Spica	1	−0·62	Meridian mark steady.
Ditto	"	"	+ 0·12	1·00	Ditto	2	+ 0·71	Tremulous.
Ditto	"	"	+ 0·11	0·49	γ Drac. and μ Sagitt.	3	−0·35	Good series of transits.
Ditto	"	"	−0·08	0·90	η Ur. Maj. and β′ Scorp.	3	−0·64	Good trans. S. of zenith.
7 Scorpii . . . δ	15 51 28·34	112 11	+ 0·40	1·00	η Urs. Maj. and Spica	1	+ 1·04	The suggested errors bold.
8 Scorpii . . . β′	15 56 43·37	109 23	+ 0·03	0·90	η Ur. Maj. and $α^2$ Lib.	2	−0·90	Good night S. of zenith.
1 Ophiuchi . . δ	16 06 29·03	93 18	−0·15	0·90	η Ur. Maj. and β′ Scorp.	3	−0·74	Safe S. of zenith.
20 Scorpii . . . σ	16 12 04·58	115 14	+ 0·11	0·87	η Urs. Maj. and Spica	1	−0·93	Too great a proportion of low stars.
21 Scorpii *N. A.* . . α	16 20 13·21	116 05	+ 0·27	0·87	η Urs. Maj. and Spica	1	−0·95	Meridian marks steady.
23 Scorpii . . . τ	16 26 33·18	117 54	−0·32	0·61	η Urs. Maj. and Spica	2	+ 0·66	Level unusually out.
Ditto	"	"	+ 0·27	0·87	Ditto	3	−0·97	Meridian marks steady.
25 Scorpii . P. 170	16 37 41·13	115 15	−0·47	0·61	η Urs. Maj. and Spica	3	+ 0·64	An initiatory night.
42 Ophiuchi . . θ	17 12 47·98	114 52	+ 0·27	1·23	Mean of 7th and 9th	2	−1·32	No low Greenw[h] star.
23 Draconis . . . β	17 27 02·39	37 35	−0·42	1·30	η Ur. Maj. and δ Aqui.	0	−0·03	Nearly all Greenw. stars

OBSERVATIONS.

Date.	Star's name.	Telescope wires. I.	II.	Centre.	IV.	V.	Mean Transit.	Clock's Error.	Daily rate.	Remarks.
		secs.	secs.	h. m. s.	secs.	secs.	s.	s.	s.	
1839 5 July	60 Ophiuchi *comp.* . . β	54·2	13·0	17 35 31·8	50·6	9·4	31·80	— 2·28	— 0·12	Rate with α Serp. 7 days after.
1838 11 May	3 Sagittarii *comp.* . . p	54·0	15·0	17 37 36·0	57·0	18·0	36·00	+ 11·81	— 0·82	Pretty steady. Rate with Antares the day before.
8 June	Ditto	12·9	33·0	17 36 54·4	15·5	36·0	54·36	— 30·45	— 2·15	Rate by Antares previous day.
11 May	33 Drac. *N. A.* . . . γ	5·5	35·0	17 53 5·0	35·0	4·7	5·04	+ 12·03	— 1·02	Zen. time. Rate by Capella.
31 Aug.	Ditto	2·7	32·6	17 53 2·6	32·5	2·3	2·54	+ 9·06	— 1·04	Rate by β Drac. previous day.
1839 5 July	13 Sagittarii *N. A.* . . μ	29·5	49·5	18 04 . .	29·5	49·5	9·50	— 2·32	+ 0·06	Assumed. Rate with α^2 Lib. 4 days before.
1838 8 June	19 Sagittarii *comp.* . . δ	27·0	48·3	18 10 9·8	31·2	52·7	9·80	— 30·54	— 2·24	Rate by Antares previous day.
27 Sept.	3 Lyræ *N. A.* . . . α	. .	. .	18 31 39·5	3·2	27·0	39·47	+ 10·87	— 2·12	Rate with α Cor. next day.
25 Nov.	Ditto	. .	15·9	18 31 39·6	3·4	27·0	39·58	+ 12·16	— 1·17	Clear and steady. Rate with itself next day.
26 „	Ditto	50·9	14·7	18 31 38·3	2·1	26·0	38·40	— 10·99	— 1·17	Clear and steady. Rate of Fomal. 3 days after.
1839 15 July	Ditto	41·3	5·0	18 31 29·0	52·5	17·0	28·96	— 3·06	— 0·17	Rate with Capella 3 days previous.
1838 8 June	27 Sagittarii *comp.* . . φ	22·7	43·6	18 35 4·3	25·0	46·0	4·32	— 30·59	— 2·29	Rate by Antares previous day.
27 Sept.	Ditto	. .	24·4	18 35 45·1	5·8	26·5	45·10	+ 10·23	— 2·44	Tremulous. Rate with ζ Aqui. next day.
3 Oct.	10 Lyræ *N. A.* . . . β	. .	. .	18 44 4·8	27·0	49·0	4·72	— 2·86	— 2·08	Rate with γ Aqui. 3 days before.
30 Aug.	34 Sagittarii *calc.* . . σ	45·6	6·5	18 45 27·2	48·0	8·8	27·22	+ 10·81	— 1·75	Horizon thick. Rate with β Ophiu. 10 days previous.
31 „	Ditto	43·0	3·8	18 45 24·7	45·5	6·4	24·68	+ 8·29	— 2·52	Bad figure. Rate with itself previous day.
27 Sept.	Ditto	44·0	5·0	18 45 25·8	46·5	7·2	25·70	+ 9·73	— 2·28	Very trem^s. Rate with γ Capr. 3 days after.
27 Sept.	14 Lyræ *calc.* . . . γ	. .	43·5	18 53 5·6	27·8	50·0	5·55	+ 10·19	— 2·17	Rate with β Lyræ 6 days after.
26 Oct.	Ditto	15·6	37·4	18 52 59·5	21·6	43·6	59·54	+ 5·89	— 2·29	Comp. by 1112 Cat. Rate with γ Cyg. 2 days before.
30 Aug.	40 Sagittarii *calc.* . . τ	21·4	42·2	18 57 3·2	24·5	45·7	3·40	+ 10·55	— 2·89	Faint. Rate with itself next day.
31 „	Ditto	19·0	40·0	18 57 1·0	22·0	43·0	1·00	+ 8·16	— 1·91	Good coincidence. Rate with μ′ Sagitt. 3 days after.
27 Sept.	17 Aquilæ *N. A.* . . ζ	32·0	51·0	18 58 10·2	29·3	. .	10·00	+ 10·12	— 2·16	Rate with β Lyræ 6 days after.
26 Oct.	Ditto	25·9	45·0	18 58 4·2	23·4	42·6	4·22	+ 4·64	— 2·08	Rate with α Aqui. 2 days before.
1839 4 July	Ditto	22·9	42·0	18 58 1·0	20·0	39·1	1·00	— 2·63	+ 0·01	Rate with δ Aqui. the previous day.

DEDUCTIONS.

Star's name.	AR. 1 Jan. 1850.	Approx. P.D.	Δ Ast. C.	Daily Azim. Dev. E.	Stars compared for daily Azim.	Wt.	Az. correction.	Remarks.
	h. m. s.	° ′	s.	s.			s.	
60 Ophiuchi . . . β	17 36 03·02	85 22	+ 0·26	0·40	γ Drac. and α Ophiu.	2	— 0·29	Tolerable night.
3 Sagittarii . . . ν	17 38 06·86	117 40	+ 0·25	0·61	η Urs. Maj. and Spica	2	+ 0·66	Irregular series.
Ditto	"	"	+ 0·10	1·23	Mean of 7th and 9th	2	— 1·36	No low Greenwich star.
33 Draconis . . . γ	17 53 07·15	38 29	— 0·18	0·61	η Urs. Maj. and Spica	0	0	Too near the zenith.
Ditto	"	"	+ 0·17	1·36	β Drac. and μ Sagitt.	0	0	A zenith star.
13 Sagittarii . . . μ′	18 04 47·28	111 06	+ 0·33	0·40	γ Drac. and α Ophiu.	0	— 0·40	Tolerable. This star agrees best with ι Boot.
19 Sagittarii . . . δ	18 11 23·13	119 54	+ 0·09	1·23	Mean of 7th and 9th	3	— 1·40	Troublesome night.
3 Lyræ . . . . α	18 31 50·07	51 21	+ 0·21	1·18	Mean, stars too close	1	+ 0·35	Greenw. stars too close.
Ditto	"	"	+ 0·32	0·50	α Cyg. and Fomal.	0	+ 0·16	Short but good night.
Ditto	"	"	+ 0·33	0·31	γ Drac. and Fomal.	1	+ 0·09	Transits irregular.
Ditto	"	"	— 0·03	0·49	γ Drac. and η Sagitt.	3	— 0·15	No perceptible collimation error.
27 Sagittarii . . . φ	18 36 17·30	117 08	+ 0·01	1·23	Mean of 7th and 9th	1	— 1·35	Tried several stars for deviation in Azim.
Ditto	"	"	+ 0·36	1·18	Mean, stars too close	1	+ 1·30	Greenw. stars too close.
10 Lyræ . . . . β	18 44 32·01	56 49	— 0·03	1·00	α Lyræ and mean	2	— 0·38	The Greenwich stars too near each other in P.D. this night.
34 Sagittarii . . . σ	18 45 57·73	116 29	+ 0·32	1·18	β Drac. and β Ophiu.	3	+ 1·20	Very few stars observed, cloudy. No perceptible error of collimation.
Ditto	"	"	+ 0·04	1·36	β Drac. and μ′ Sagitt.	3	+ 1·48	Level was applied.
Ditto	"	"	+ 0·03	1·18	α Lyræ and mean	1	+ 1·29	Level error W. + 0·57.
14 Lyræ . . . . γ	18 53 19·34	57 31	— 0·32	1·18	Mean of 27th and 28th inst.	0	+ 0·47	Stars tremulous.
Ditto	"	"	+ 1·22	1·25	α Aqui. and mean	0	+ 0·50	Epps made the Azim. dev. 1s·35, but does not say how.
40 Sagittarii *calc.* . . τ	18 57 34·12	117 53	+ 0·14	1·18	β Drac. and β Ophiu.	3	+ 1·28	Returned the Axis.
Ditto	"	"	— 0·05	1·36	β Drac. and μ′ Sagit.	2	+ 1·51	Changeable night.
17 Aquilæ. . . . ζ	18 58 30·44	76 21	— 0·10	1·18	α Aqui. and β Aqui. balanced.	3	+ 0·75	Azim. dev. difficult.
Ditto	"	"	+ 0·14	1·25	α Aqui. and mean	2	+ 0·79	Tried several eye-pieces.
Ditto	"	"	— 0·12	0·63	α Cyg. and α^2 Capric.	2	— 0·40	Collimation too little to correct for.

OBSERVATIONS.

Date.	Star's name.	Telescope wires. I.	II.	Centre.	IV.	V.	Mean Transit.	Clock's Error.	Daily rate.	Remarks.
		secs.	secs.	h. m. s.	secs.	secs.	s.	s.	s.	
1838 31 Aug.	57 Draconis *calc.* . δ	4·8	53·0	19 12 41·4	29·8	18·0	41·40	+ 9·81	—1·96	AR. from 1112 Cat. Rate with γ Draconis 3 days after.
28 Sept.	Ditto	2·0	50·5	19 12 38·5	27·0	15·0	38·60	+ 9·10	—2·02	Very tremulous. Rate with α Ceph. 2 days after.
26 Oct.	Ditto	57·0	45·5	19 12 33·7	22·0	10·5	33·74	+ 5·88	—2·26	Rate with γ Drac. 4 days after.
1839 4 July	Ditto	. .	42·0	19 12 30·2	19·0	7·0	30·07	— 2·32	—0·14	Rate with α Urs. Maj. 9 days after.
1838 31 Aug	30 Aquilæ *N. A.* . δ	54·3	13·0	19 17 31·5	50·0	8·5	31·40	+ 8·97	—2·05	Rate with ζ Aquil. 8 days before.
26 Oct.	Ditto	48·8	7·5	19 17 26·2	44·8	3·5	26·16	+ 4·49	—2·24	Rate with itself 0 days before.
27 Sept.	52 Sagittarii *calc.* . h^2	22·5	43·0	19 27 3·5	24·0	44·5	3·50	+ 9·71	—2·21	Rate with μ' Sagittar. 7 days before.
27 Sept.	50 Aquilæ *N. A.* . γ	8·0	27·0	19 38 46·0	4·9	23·9	45·98	+ 10·06	—2·15	Rate with itself 6 days before.
28 „	Ditto	6·0	24·8	19 38 43·8	. .	21·0	43·62	+ 7·72	—2·34	Cloudy. Rate with itself yesterday.
30 „	Ditto	1·6	24·4	19 38 39·2	58·0	17·0	39·24	+ 3·37	—2·18	Rate with itself 2 days before.
26 Oct.	Ditto	2·8	21·1	19 38 40·0	59·0	17·8	40·04	+ 4·60	—2·26	Rate with itself 6 days before.
26 Nov.	Ditto	7·7	26·5	19 38 45·3	4·3	23·0	45·36	+ 10·29	—1·13	Rate with itself yesterday.
1839 4 July	Ditto	58·4	. .	19 38 36·2	. .	14·0	36·20	— 3·10	—0·02	Rate with β Libræ 11 days after.
1838 31 Aug.	53 Aquilæ *N. A.* . α	26·7	45·5	19 43 4·3	23·1	42·0	4·32	+ 8·73	—2·10	Rate with β Ophiu. 3 days after.
27 Sept.	Ditto	27·2	46·1	19 43 5·0	24·0	42·8	5·02	+ 9·77	—2·20	Rate with itself 6 days before.
28 „	Ditto	. .	44·0	19 43 3·0	. .	40·7	2·98	+ 7·74	—2·03	Cloudy. Rate with itself yesterday.
30 Sept.	Ditto	21·0	39·9	19 42 58·8	17·6	36·4	58·74	+ 3·53	—2·20	Cloudy. Rate with itself 2 days before.
24 Oct.	Ditto	26·2	44·8	19 43 3·6	22·3	41·2	3·62	+ 8·80	—2·28	Cloudy. Rate with itself 4 days previous.
25 Nov.	Ditto	28·2	47·0	19 43 5·8	24·5	43·1	5·72	+ 11·29	—1·07	Steady. Rate with itself next day.
26 „	Ditto	27·0	45·8	19 43 4·7	23·5	42·2	4·64	+ 10·22	—1·68	Rate with itself 12 days after.
1839 4 July	Ditto	18·0	37·0	19 42 55·5	15·0	33·0	55·70	— 2·96	+ 0·68	Rate with β Ophiu. next day.
1838 31 Aug.	60 Aquilæ *N. A.* . β	55·6	14·2	19 47 32·8	51·4	10·2	32·84	+ 8·59	—2·15	Rate with β Ophiu. 3 days after.
24 Oct.	Ditto	54·8	13·4	19 47 32·0	50·8	9·5	32·10	+ 8·60	—2·30	Cloudy. Rate with itself 4 days before.
26 „	Ditto	50·6	9·2	19 47 27·9	46·5	5·2	27·88	+ 4·40	—2·10	Rate with itself 2 days before.
1839 4 July	Ditto	47·0	5·5	19 47 24·0	43·0	2·0	24·30	— 3 01	+ 0·73	Rate with β Ophiu. next day.

DEDUCTIONS.

Star's name.	AR. 1 Jan. 1850.	Approx. P.D.	Δ Ast. C.	Daily Azim. Dev. E.	Stars compared for daily Azim.	Wt.	Az. correction.	Remarks.
	h. m. s.	° ′	s.	s.			s.	
57 Draconis . . . ð	19 12 29·56	22 36	—0·32	1·36	β Drac. and μ′ Sag.	3	—0·95	Level error E. 0·97.
Ditto	″	″	—0·39	1·30	ð Drac. and ð Aqui.	3	—0·97	The only unknown star to-night.
Ditto	″	″	—0·30	1·25	α Aqui. and mean.	3	—0·88	Observed Encke's comet also.
Ditto	″	″	—0·66	0·63	α Cyg. and α Capric.	3	+ 0·44	Shifted the axis E. to W.
30 Aquilæ . . . ð	19 17 55·46	87 11	+ 0·31	1·36	β Drac. and μ Sagitt.	1	+ 1·02	Observed 82 Urs. Min.
Ditto	″	″	+ 0·12	1·25	α Aqui. and mean.	1	+ 0·94	Regulated chronometers.
52 Sagittarii . . . h^2	19 27 34·13	115 13	+ 0·06	1·18	Mean of 27th and 28th inst.	3	+ 1·26	Level W. + 0·57.
50 Aquilæ . . . . γ	19 39 07·57	76 45	—0·07	1·18	α Ly. and β Aqui. bald. 28th	3	+ 0·78	N.A. differs from A.S.C. which gives AR. less by ·13.
Ditto	″	″	—0·14	1·30	η U. Maj. and ð Aqui.	2	+ 0·83	Transits very irregular.
Ditto	″	″	+ 0·14	0·44	α Ceph. and β Aqua.	2	+ 0·29	From P.D. 97° to 107° clock err. too small.
Ditto	″	″	+ 0·06	1·25	α Aqui. and mean.	2	+ 0·86	Obsd. from P.D. 22° to 115°, but results very irregular.
Ditto	″	″	—0·22	0·31	γ Drac. and Fomalt.	1	+ 0·19	Clock errors very irreg. to-night.
Ditto	″	″	—0·60	0·63	α Cyg. and α^2 Capric.	0	—0·40	Obsd. by Mr. Mann this night.
53 Aquilæ . . . . . α	19 43 27·67*	81 32	+ 0·03	1·36	β Drac. and μ′ Sag.	2	+ 0·95	Bad figure.
Ditto	″	″	—0·30	1·18	α Lyr. and β Aqui. and 28th.	1	+ 0·83	α Aqui. gave more Azim. dev.
Ditto	″	″	—0·03	1·30	ð Drac. and ð Aqui.	3	+ 0·50	Though many Gr. stars, results irregular.
Ditto	″	″	+ 0·34	0·44	α Ceph. and β Aqua.	2	+ 0·32	Results irregular.
Ditto	″	″	+ 0·11	1·30	α Cyg. and α^2 Capric.	3	+ 0·91	Only one unknown star.
Ditto	″	″	—0·19	0·50	α Cyg. and Fomalt.	2	+ 0·35	Good series to-night.
Ditto	″	″	—0·27	0·31	γ Drac. and Fomalt	1	+ 0·21	Long but irreg. series.
Ditto	″	″	—0·42	0·63	α Cyg. and α^2 Capric.	0	—0·44	Obs. by Mr. Mann.
60 Aquilæ . . . . β	19 47 56·54	83 59	—0·08	1·36	β Drac. and μ Sagitt.	2	+ 0·98	Tolerably regular.
Ditto	″	″	—0·07	1·30	α Cyg. and α Capric.	3	+ 0·94	Only 5 transits to-night.
Ditto	″	″	—0·05	1·25	α Aqui. and mean.	3	+ 0·90	Az. dev. by Epps 1·35.
Ditto	″	″	—0·46	0·63	α Cyg. and α^2 Capric.	0	—0·45	Eye-piece changed.

OBSERVATIONS.

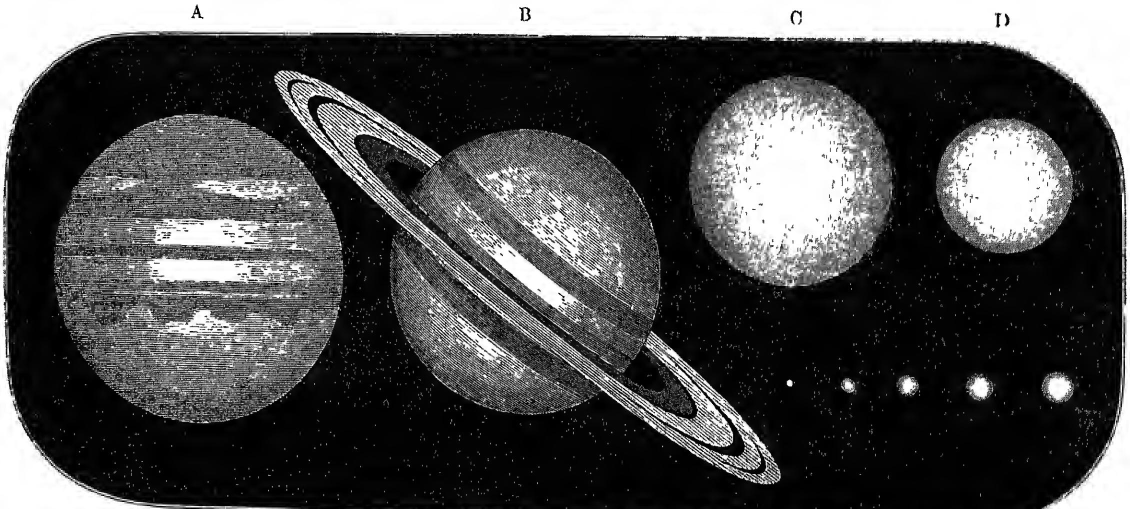
Date.	Star's name.	Telescope wires. I.	II.	Centre.	IV.	V.	Mean Transit.	Clock's Error.	Daily rate.	Remarks.
		secs.	secs.	h. m. s.	secs.	secs.	s.	s.	s.	
1838 31 Aug.	62 Sagittarii *calc.* . c	11·1	32·2	19 52 53·2	14·3	35·5	53·26	+ 08·26	— 1·95	Hazy. Rate with μ' Sagitt. 3 days after.
27 Sept.	Ditto	12·2	33·0	19 52 54·1	15·1	36·0	54·08	+ 09·42	— 2·02	Rate with γ Aqui. 3 days after.
1839 4 July	65 Aquilæ *calc.* . . . θ	23·0	. .	20 02 59·8	17·8	37·1	59·97	— 02·75	+ 0·47	Rate with β Ophiu. next day.
1838 25 Nov.	6 Capricorni *N.A.* . α^2	39·0	58·0	20 09 17·0	. .	. .	17·08	+ 11·15	— 1·80	Rate with β Ceti 11 days after.
26 „	Ditto . . α^1	14·2	32·5	20 08 51·8	11·0	30·2	51·94	+ 10·00	— 1·72	Rate with ι Orionis 7 days after.
1839 4 July	9 Capricorni *calc.* . β^2	. .	39·0	20 11 58·3	17·4	37·0	58·28	— 02·53	+ 0·25	Rate with β Ophiu. next day.
1838 24 Oct.	37 Cygni *calc.* . . . γ	47·5	11·6	20 16 35·8	. .	24·3	35·84	+ 00·97	— 2·26	Rate with α Cyg. 5 days after.
1839 4 July	Ditto	39·4	3·0	20 16 27·6	52·0	16·0	27·60	— 01·63	— 0·48	Rate with γ Boot. next day.
1838 1 Nov.	50 Cygni *N. A.* . . . α	56·0	22·0	20 35 48·2	14·4	40·6	48·24	— 07·64	— 2·20	Glaring figure. Rate with γ Cyg. 8 days before.
26 Oct.	16 Capricorni *calc.* . ψ	55·3	10·0	20 36 36·6	57·2	17·8	36·58	+ 03·73	— 2·32	Rate with α^2 Capric. 6 days before.
1839 4 July	53 Cygni *calc.* . . . ϵ	57·8	19·8	20 39 42·0	4·6	26·5	42·14	— 01·70	— 0·41	Rate with γ Boot. next day.
1838 26 Oct.	34 Capricorni *calc.* . ζ	50·8	11·0	21 17 31·2	51·5	11·8	31·26	+ 03·59	— 2·00	Rate with α^2 Capric. 6 days before.
30 Sept.	40 Capricorni *calc.* . γ	34·0	53·5	21 31 13·0	32·6	51·8	12·98	+ 03·04	— 2·00	Rate by α^2 Capric. 3 days before.
26 Oct.	Ditto	34·6	54·0	21 31 13·5	33·0	52·5	13·52	+ 03·92	— 2·28	Rate with α^2 Capric. 6 days before.
29 Oct.	8 Pegasi *N. A.* . . ϵ	36·8	55·5	21 36 14·2	33·0	51·8	14·26	— 02·16	— 2·19	Rate with α Aqui. 5 days before.
30 Sept.	49 Capricorni *calc.* . δ	33·5	52·7	21 38 12·1	31·5	50·9	12·14	+ 03·23	— 2·35	Rate with α^2 Capric. 12 days after.
24 Nov.	Ditto	41·8	1·2	21 38 20·6	40·0	59·3	20·58	+ 12·36	— 1·52	Rate with α^2 Capric. 12 days before.
24 Nov.	51 Capricorni *calc.* . μ	4·0	23·2	21 44 42·4	1·5	20·8	42·38	+ 12·81	— 1·48	Rate with α^2 Capric. 12 days before.
30 Sept.	34 Aquarii *N. A.* . α	57·0	15·5	21 57 34·0	52·6	11·2	34·06	+ 03·11	— 2·37	Rate with itself 12 days after.
29 Oct.	Ditto	51·0	9·7	21 57 28·2	46·7	5·2	28·16	— 02·58	— 2·24	Rate with α Aqui. 5 days before.
3 Sept.	33 Aquarii *calc.* . . ι	18·0	27·0	21 57 46·2	5·6	24·8	46·32	+ 01·85	— 2·06	White clouds. Rate with μ' Sag. 7 days after.
24 Nov.	Ditto	17·6	36·6	21 57 55·8	15·1	34·3	55·88	+ 12·16	— 1·53	Rate with α^2 Capric. 12 days before.
3 Sept.	57 Aquarii *calc.* . . σ	31·8	50·8	22 22 9·7	28·5	47·3	9·62	+ 01·91	— 2·08	White clouds. Rate with μ' Sag. 7 days after.

DEDUCTIONS.

Star's name.		AR. 1 Jan. 1860.	Approx. P.D.	Δ Ast. S. C.	Daily Azim. Dev. E.	Stars compared for daily Azm.	Wt.	Az. correction.	Remarks.
		h. m. s.	° ′	s.	s.			s.	
62 Sagittarii . . .	e	19 53 25·42	118 07	+ 0·12	1·36	β Drac. and μ′ Sagit.	2	+ 1·51	Tried various deviations.
Ditto		"	"	— 0·10	1·18	α Lyræ and β Aqui. balanced 28th.	2	+ 1·32	Very irregular results.
65 Aquilæ . . .	θ	23 03 33·69	91 16	— 0·15	0 63	α Cyg. and α′ Capric.	1	— 0·50	Observed by Dr. Lee.
6 Capricorni . .	α^2	20 09 43·60	103 00	— 0·17	0·50	α Cyg. and Fomalt.	2	+ 0·47	Good series to-night.
Ditto . .	α^1	"	"	— 0·40	0·31	γ Drac. and Fomalt.	1	+ 0·28	An irregular night, but + 10·70, mean err. by 7 Gr. stars.
9 Capricorni . .	β^2	20 12 34·54	105 16	+ 0·17	0·63	α Cyg. and α^2 Caprio.	1	— 0·60	Observed by Dr. Lee.
37 Cygni	γ	20 16 50·26	50 14	+ 0·77	1·30	α Cyg. and α^2 Capric.	0	+ 0·37	The only unknown star.
Ditto		"	"	+ 0·64	0·63	Ditto	0	— 0·17	Observed by Mr. Mann.
50 Cygni	α	20 36 18·09	45 15	— 0·03	1·35	α Pers and α Capric.	2	— 0·23	Disagreed with its neighbours. Level W. 0″·05.
16 Capricorni . .	ψ	20 37 12·88	115 48	— 0·24	1·25	α Aqui. and mean.	2	+ 0·69	Disagreed with ζ Capric.
53 Cygni	ε	20 40 07·59	56 36	+ 0·04	0·63	α Cyg. and α Capric.	0	— 0·24	Observed by Dr. Lee.
34 Capricorni . .	ζ	21 18 05·34	113 03	— 0·48	1·25	α Aqui. and mean.	3	+ 0·66	Disagreed with ψ Capric.
40 Capricorni . .	γ	21 31 46·06	107 20	+ 0·14	0·44	α Ceph. and β Aqua.	1	+ 0·44	Unsteady night.
Ditto		"	"	— 0·19	1·25	α Aqui. and mean.	2	+ 0·61	Trustworthy observations.
8 Pegasi	ε	21 36 48·04	80 59	+ 0·09	1·28	α Cyg. and α Capric.	2	— 0·86	Good regular night.
49 Capricorni . .	δ	21 38 44·89	106 48	+ 0·33	0·44	α Ceph. and β Aqua.	1	— 1·44	Merid. pos same as yesterday.
Ditto		"	"	— 0·11	0·12	γ Ceph. and Fomal.	2	+ 0·24	Position of object glass rectified.
51 Capricorni . .	μ	21 45 05·92	104 15	+ 0·34	0·12	γ Ceph and Formal.	1	+ 0·11	Level E. 0″·50. No app. collim.
34 Aquarii . . .	α	21 58 04·52	91 02	+ 0·14	0·44	α Ceph. and β Aqua.	3	+ 0·36	Disagrees with its neighbours.
Ditto		"	"	— 0·34	1·28	α Cyg. and α Capric.	1	— 1·02	Not in accordance with the rest to-night.
33 Aquarii . . .	ι	21 58 19·93	104 35	— 0·18	1·00	Mean and μ′ Sagit.	2	+ 0·95	Stars tremulous.
Ditto		"	"	— 0·29	0·12	γ Ceph. and Formal.	0	+ 0·12	Position of instrument good.
57 Aquarii . . .	σ	22 22 42·38	101 26	+ 0·04	1·00	Mean and μ′ Sagit.	2	+ 0·92	Level E. 0″·88.

OBSERVATIONS.

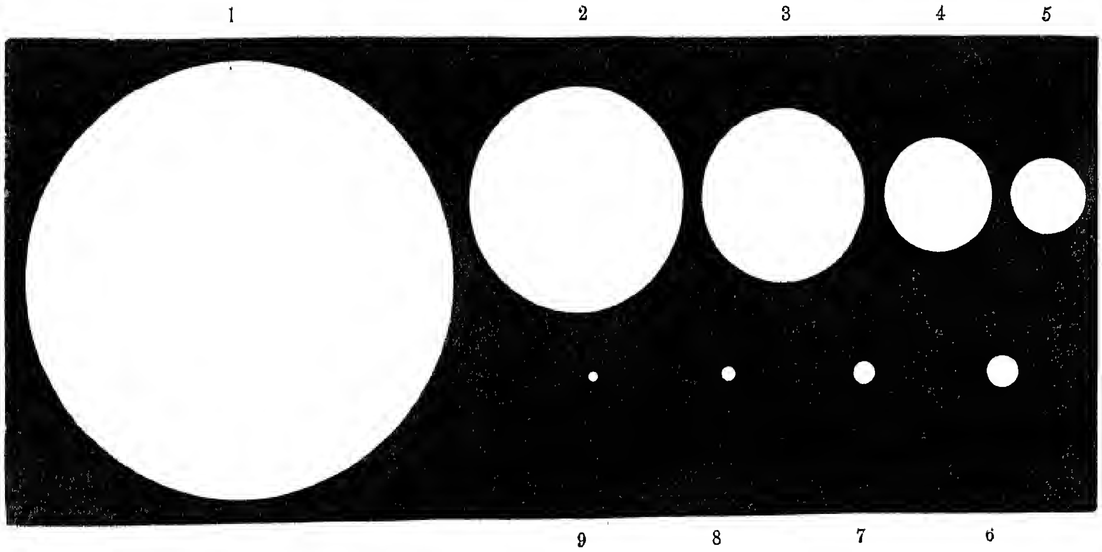
Date.	Star's name.	Telescope wires. I.	II	Centre.	IV.	V.	Mean Transit.	Clock's Error.	Clock's Daily rate.	Remarks.
		secs.	secs.	h. m. s.	secs.	secs.	s.	s.	s.	
1838 24 Nov.	42 Pegasi *N.A.* . . ζ	0·2	19·0	22 33 38·0	57·0	16·0	38·04	+ 12·40	— 1·52	Rate with ι Peg. 12 days before.
3 Sept.	73 Aquarii *calc.* . . λ	37·5	56·3	22 44 15·0	33·8	52·5	15·02	+ 2·11	— 2·15	Rate with α Serp. 8 days before.
24 Nov.	Ditto	47·0	5·8	22 44 24·5	43·3	2·0	24·52	+ 11·92	— 1·55	Rate with α^2 Capric. 12 days before.
25 Nov.	Ditto	46·0	4·8	22 44 23·5	42·2	1·0	23·50	+ 10·98	— 0·94	Very tremulous. Rate with itself previous day.
24 Nov.	54 Pegasi *N.A.* . . . a	19·0	38·0	22 56 57·0	16·2	35·5	57·14	+ 12·66	— 1·34	Rate with itself next day.
25 ,,	Ditto	17·3	36·6	22 56 55·8	15·0	34·2	55·78	+ 11·32	— 1·03	Clear. Rate with γ Aqui. next day.
3 Sept.	90 Aquarii *calc.* . φ	23·5	42·2	23 6 1·0	19·8	38·5	1·00	+ 1·67	— 2·61	Very tremulous. Rate with itself next day.
29 Oct.	Ditto	19·0	37·8	23 5 56·5	15·1	33·9	56·46	— 2·82	— 2·37	Rate with δ Aqui. 3 days before.
3 Sept.	Piscium *calc.* . . χ	6 0	24·5	23 18 43·0	1·5	20·0	43·00	+ 1·94	— 2·22	Bad. A double star. Rate with β Aqui. 3 days before
25 Nov.	20 Piscium ☽ . . n	13 6	32 2	23 39 50·8	9·5	28·0	50·82	+ 11·25	— 0·85	Very hazy. Rate with itself next day.
26 ,,	Ditto	12·6	31·4	23 39 50·0	8·6	27·2	49 96	+ 10·40	— 1·28	Rate with α Aqua 3 days after
29 Oct.	29 Piscium *calc.* . q	55·0	13·6	23 53 32·2	50·8	9·5	32·22	— 2·71	— 2·40	Rate with δ Aqui. 3 days before.
26 Nov.	Ditto	6·8	25 5	23 53 44·1	2·8	21·4	44·12	+ 9·40	— 0·94	Rate with α Aqua. 3 days after



Comparative magnitudes of the primary planets, according to the elements given in the *Cycle* and in this Volume. A, Jupiter; B, Saturn; C, Neptune D, Uranus; E, Earth; F, Venus; G, Mars; H, Mercury; I, Ceres.

DEDUCTIONS.

Star's name.	AR. 1 Jan. 1850.	Approx. P.D.	△ Ast. S.C.	Daily Azim. Dev. E.	Star's compared for daily Azim.	Wt.	Az. correction.	Remarks.
	h. m. s.	° ′	s.	s.			s.	
42 Pegasi . . . . ζ	22 33 58·56	79 57	— 0·05	0·12	γ Ceph. and Fomal.	1	+ 0·87	Position of instrument good.
73 Aquarii . . . λ	22 44 46·79	98 23	+ 0·13	1·00	Mean and μ′ Sagit.	1	+ 0·87	White clouds.
Ditto	"	"	— 0·50	0·12	γ Ceph. and Fomal.	2	+ 0·10	Level and collimation good.
Ditto	"	"	— 0·24	0·50	α Cyg. and Fomal.	0	+ 0·44	Meridional position continues good.
54 Pegasi . . . . α	22 57 17·21	75 36	+ 0·22	0·12	γ Ceph. and Fomal.	1	+ 0·07	Sky clouding over.
Ditto	"	"	— 0·04	0·50	α Cyg. and Fomal.	3	+ 0·32	Position of object-glass rectified.
90 Aquarii . . . φ	23 6 32·85	96 52	— 0·14	1·00	Mean and μ′ Sagit.	1	+ 0·86	Level E. + 0″·88.
Ditto	"	"	— 0·39	1·28	α Cyg. and α Capric.	0	— 1·10	Returned to eye-piece 3.
Piscium . . . . χ′	23 19 13·61	89 34	+ 0·21	1·00	Mean and μ Sagit.	0	+ 0·78	Uncertain sky.
20 Piscium . . . υ	23 40 13·30	93 36	+ 0·03	0·50	α Cyg. and Fomal.	2	+ 0·42	By NA. 23, 39, 39·56.
Ditto	"	"	+ 0·15	0·31	γ Drac. and Fomal.	2	+ 0·25	Thermometer in observatory 35°.
29 Piscium . . . η	23 54 7·39	93 52	— 0·25	1·28	α Cyg. and α Capric.	1	— 1·06	Encke's comet observed.
Ditto	"	"	— 0·85	0·31	γ Drac. and Fomal.	2	+ 0·25	Clock vibrating 17·5 each side, in summer = 20·0.



Apparent magnitudes of the Sun, as seen from the different Planets. 1, from Mercury; 2, Venus; 3, the Earth; 4, Mars; 5, Ceres; 6, Jupiter; 7, Saturn; 8, Uranus; 9, Neptune. (See *Cycle*, Vol. I. page 206.)

CHAPTER VI.

DOUBLE STARS RE-MEASURED AT HARTWELL.

" He (NEWTON) took his ardent flight
Through the blue infinite; and every star,
Which the clear concave of a winter's night
Pours on the eye, or astronomic tube,
Far stretching, snatches from the dark abyss;
Or such as further in successive skies
To fancy shine alone, at his approach
Blaz'd into suns, the living centre each
Of an harmonious system: all combin'd,
And rul'd unerring by that single power,
Which draws the stone projected to the ground."

THOMSON.

In my work upon Celestial Objects (vol. i. pp. 285—304), I have given a condensed view of the highly interesting department of Compound Stars, which work can be referred to by many: but as it will not be accessible to all who may honour these pages, a prefatory word or two may be considered necessary, towards their comprehending the glimpse which will be afforded of the wondrous results of the OMNIPOTENT FIAT:—

" Then speak thy humblest thanks; that thus 'tis giv'n
To thee (a worm, a mite, an atom! plac'd
On this small Earthly Ball, to th' universe
Like dust of balance, or the bucket's drop,)
To read, to trace, to know HIS glorious works."

Our statement will be recollected, that a dark and silent void space extends above, below, and around our system, which has been successfully pierced by the

force of genius and art: this, indeed, is one of the triumphs of Man's intellect, for the abyss is so inconceivably vast, as absolutely to stun contemplation, the distance to the nearest star (*α Centauri*) being estimated at not less than twenty-one million millions of miles. But the void which thus awfully separates us from the sidereal heavens is known to expand towards other bodies in such a ratio, that the remoteness of *α Centauri* is comparatively small; and imagination is confounded, even with the facts demonstrated. Among the assemblage of the stars which deck the heavens, in numbers so vast as to be absolutely innumerable, many individuals appear as single objects to the naked eye, which are found really to consist of two, or more, under some of the finest and most delicate forms of vision conceivable. Most of these, from their enormous distance, appear to be so close to one another, that it requires telescopes of great perfection to separate them. The practical astronomy of these elegant objects originated in the unprecedented labours of the elder Herschel at Slough, and constituted one of his greatest bequests to science. When demanding, "What's in a name?" the Swan of Avon assuredly was not dreaming of science, whose advances are so greatly accelerated and secured by regular nomenclature and classification. A worthy friend of mine, versed in the annals of astronomy, though he approves of my having given Ptolemy's διπλοῦς, in Τοξότης, as the earliest mention of double stars, insists that the name was not at all unknown in 1754, for the Père Feuillée had described *α Centauri* as double in 1709. My assertion, however (Cycle, I. p. 286), is, that it did not obtain a place in Sir John Hill's *Astronomical Dictionary;* and in truth Feuillée's words are merely, "Je trouvai cette étoile composée de deux, dont l'une est de la troisième grandeur, et l'autre de la quatrième:" La Condamine was nearer the mark in 1749, when, in describing the same star, he used the expression, "etiam duplex est." Yet nearly in the middle of the seventeenth century, Riccioli had found that under the telescope Mizar consisted of two stars in close justa-position; and in 1664 Hook detected the duplicity of Mesartim (Cycle, II. p 44). But certainly the term, as expressing twin stars, had not widely obtained; and Michell—a discerning

inquirer into the cosmical structure of the heavens—was the first who suggested, in 1767, that the proximity in such instances might be owing to a physical connexion existing between them (Cycle, I. pp. 289, 290). Still, general interest was not awakened, till the astronomer of Slough published his celebrated paper in the Philosophical Transactions for 1782.

The mantle of Michell disappeared for a time: and when Herschel commenced the inquiry, the circumstance of a star being double was hastily accounted for, by supposing one star to be situated at an immense distance beyond the other. It was therefore presumed to be only optically double, or the two components so nearly in a line with our eye as scarcely to subtend a perceptible angle, yet without any real proximity between them. But owing to his sagacious perseverance, especially as regarded the observations of that beautiful double-star Castor, on an interval of twenty-five years (1778 to 1803), the relative places of the components were ascertained to vary; and the changes detected had no relation to the orbital position of the Earth. In the course of the scrutiny which he prosecuted, it was proved that in various cases the stars were physically binary, and that the companion must actually revolve about its primary. Thus the wonderful truth opened to view, that two suns, each self-luminous and probably with an attendant train of planets, were gyrating round their common centre of gravity under the same dynamical laws which govern the solar system: that is, not precisely like our planets round one great luminary, but where each constituent with its accompanying orbs revolves around an intermediate point, or mystic centre. This is a great fact; and an astronomical revelation which, in all probability, Newton himself never contemplated.

These extensive celestial examinations brought Michell into the field again; and in the communication which appears in the Philosophical Transactions for 1784 (Cycle I. page 290), he expresses his conviction that double and triple stars are so many systems liable to be affected by their mutual gravitation; and he considered it as not unlikely that the periods of the revolutions of some of these about their primaries might one day be dis-

covered. But in mentioning the perseverance of Herschel, and the sagacity of Michell, I must not forget the *palmam qui meruit ferat* axiom: for the merit of having first given a method of determining, from observation, the form of the orbit of a binary star, certainly belongs to M. Savary. It is true that Sir John Herschel, struck with the idea that gravitation must be the law of motion in those distant regions, recommended constant observation upon ξ Ursæ Majoris so far back as 1825; but Savary was the first who produced a regular set of elements for the same from calculation. These he published in 1830, shewing that the *comes* moved in an ellipse precisely like the planets of our own system, though of a more varied excentricity.

The field is peculiarly attractive to the amateur or extra-meridian astronomer, and was therefore invaded from my station at Bedford. Having thus become deeply interested in stellar physics, and therefore anxious to obtain repeated observations of various objects enrolled in my Cycle, it became desirable for me to repair to Hartwell—whither my large equatoreal had been removed—as often as these came into apparition; and it were ungrateful to omit adding, that the observatory was always most obligingly kept in readiness for my immediate and particular service. The measures and re-measures thus obtained with my former instruments are consequently all taken with the same eye, micrometers, and object-glass, and mostly with the same eye-piece power; and the diurnal motion of the earth was eliminated by the same practice in the use of the Æ-handle, or the equable movement of the equatoreal clock, as manipulated at Bedford. By these means, whatever optical or other bias might obtain, the results are assuredly comparable with each other, *inter se*, in evidence of angular movement and changes of distance, indicated by the operations of different nights and different years.

Some of these re-examinations have already appeared in my Cycle, which was published in 1844; but it is proper to reprint them here, together with the latest Bedford measures and those since observed, so as to yield a *colpo d'occhio* of motion or fixity in the several lapsed times. They are arranged in the manner already explained in that work (Vol. I. pages 426—8); so that

I need only remind the reader who may be unable to make reference to it, that the weights attached to the distances and to the angles of position—as the relative situation of two component stars in respect to the fixed circles of the sphere is termed—depend, according to the observer's judgment, on the number and presumed goodness of the observations. Thus, while 1 indicates an indifferent single measure, a careful guess, or a mere estimation, 10 is used to express that the astrometer is entirely satisfied with all the conditions of the operation. The last is a weight but too rarely obtainable, and indeed beyond my registers; still, from having full leisure and a free choice of objects and opportunity, I was generally enabled to give the Hartwell measures high weights. These are principally useful in grouping the data for investigating the orbits of those which are binary. Before submitting the following list of the double-stars thus re-observed, a few preliminary statements are necessary here, though somewhat iterative to those who possess my Cycle of Celestial Objects.

Respecting the magnitudes of stars, although they have been attended to by Herschel, Dawes, Argelander, Pogson, and others, so much of the classification as yet is arbitrary and conventional, that I have little to add to my former statement (Cycle I. pages 271—2). But I may repeat, it was on account of the besetting difficulties of the methods hitherto used for estimations of brightness, and feeling that the proposition which would satisfy every point had not yet been made, that I adopted the size of the primary from Piazzi; and assuming that as a point of departure, the other stars in the field were relatively registered. This manner of proceeding cannot be radically bad, because, on comparison with other standards, my given magnitudes have been approved and accepted: thus my early correspondent Dawes, whose method is before the astronomical public, in answer to some of my inquiries on the subject, observes, under date 10th October, 1857:—

"I cannot find that I have ever gauged the magnitudes of the small stars between the doubles of ε Lyræ; but the small star which follows and makes an obtuse angle with them, I gauge of 9·5 mag.

"It does not appear to me that there is much difference in the acuteness of your eye and mine; and if this be correct, your minimum with 5·9 inches aperture = 11·3 mag. of my scale. This converted into Herschel's theoretical scale = 11·3 + 11·3 — 6·0, = 22·6 — 6·0 = 16·6 mag. which differs very little from what you have considered your *minimum visibile.* The reduction therefore of your scale to mine (supposing your ratio to be the same as Herschel's, viz. $\frac{1}{2}$) is easy, the formula being simply $D = \frac{\text{Sm} + 6\cdot 0}{2}$.

"On September 20th, I looked at that pretty double-star, Cycle 845, and gauged the magnitudes; A = 8, B = 10·3. In this case, for B we have $10\cdot 3 \times 2 - 6\cdot 0 = 20\cdot 6 - 6 = 14\cdot 6$, as your theoretical magnitude deduced from my gauging. Your estimate is 15. Herschel's 18 is out of the question. Struve's is 10·8; which, treated in the same way as my own, gives 21·6 — 6 = 15·6,—so that your estimation is nearly a mean between Σ's and my gauge, when reduced to your scale. As to β Pegasi, A 2 and B 10·5 (*gauged*). Here again our agreement is *perfect* in the *actual brightness* of the small star; for $10\cdot 5 \times 2 - 6 = 15$, which is your estimation.

"I cannot find that I have a gauge of the brighter small star about a minute and a half north preceding α Cassiopeæ. I perceive that you assign to it about 10·5. I will take an early opportunity of examining it,—though for accurate gauging, it is rather too near its bright neighbour.

"On the brighter small stars, namely, down to my 9th magnitude, I don't think we differ much in our scale, or ratio of progression. *E. g.* 10 Arietis, I have gauged A = 6·5; B = 8·5: which are precisely your own numbers. So again in ι Cassiopeæ, Cycle 97, I have A = 5; B = 7; C = 9: the two smaller being rated just the same by yourself.

"I will look over my manuscript catalogue, and make out a list of stars I have gauged, and which are also found in your Cycle: but with the exception of the *well-known binaries*, we have not observed many of the same stars,—our object in observing having been very different in respect of the others."

Thus much for the relative brightness of the pairs and groups compounded in my operations, in the course of which I have followed nearly the same order; and, although I feel rather uncertain as to whether we are yet in the best possible walk of astrometry, I have little to add to my former remarks on telescopes and micrometers. But I must throw out a crumb of comfort to the wight who sighs for the possession of the giant instruments now so frequently met with, for I can, from my own knowledge, assure him of a harvest of excellent results by a diligent use of those of more moderate dimensions, well-figured and corrected. Such for instance is the 5-foot refractor above described; but he who possesses a tool of this size in a state of perfection, ought never to teaze it with the higher powers, except under

obvious necessity; for the greater the magnifier of eye-pieces, the greater the invasion of the illuminating power of the telescope,—the light being lessened in the inverse ratio of the squares of the powers. In many instances of gazing, however, the achromatic concave lens, described in the Cycle (Vol. I. pages 342—3), can be used with great benefit; and indeed its advantages in certain cases of measuring were such, that I designated it the Macro-micrometer, under which name it appears in Dawes's registers, and in Sir John Herschel's Cape Observations. Still, whatever powers of eye-pieces, negative or positive, are used, the ultimate results obtained by present means and methods are perhaps chargeable with an instrumental proportion of excess or defect, a personal equation, and a too sensible amount of probable error.

FESTINA LENTE is the appropriate motto for an amateur astronomer; for it is by hastening slowly that he will accomplish valuable work—quality, not quantity, being the actual desideratum. With powerful instruments, a steady hand, and a practised eye, most celestial objects can be immediately attacked; but there are crucial cases which do not admit of sudden assault, especially those in which there is an absolute necessity for the actual focus being positively found. Instead of being impatient under difficulties, as I have known a friend or two to be, the tyro cannot do better than resort to the advice of the elder Herschel as to adjusting the telescope and preparing the eye for an accurate examination of the closer double-stars. For this purpose he recommends the observer to previously conform his instrument with the utmost delicacy on a star known to be single, of as nearly as possible the same altitude, magnitude, and colour, as the body proposed to be examined. He then continues:—

"As these are some of the finest, most minute, and most delicate objects of vision I ever beheld, I shall be happy to hear that my observations have been verified by other persons; which I make no doubt the curious in astronomy will soon undertake. I should observe that, since it requires no common stretch of power and distinctness to see these double stars, it will not be much amiss to go gradually through a few preparatory steps of vision, such as the following. When η Coronæ Borealis (one of the most minute double stars) is proposed to be viewed, let the telescope be some time before directed to α Geminorum, or if not in view to either of the following stars: ζ Aquarii, μ Draconis, ρ Herculis, α Piscium, or the curious *double*-double star ϵ Lyræ. These

should be kept in view a considerable time, that the eye may acquire the habit of seeing such objects well and distinctly. The observer may next proceed to ξ Ursæ Majoris, and the beautiful treble star in the right fore-foot of Monoceros; after these to ι Bootis, which is a fine miniature of α Geminorum, to the star preceding α Orionis, and to n Orionis. By this time both the eye and the telescope will be prepared for a still finer picture, which is η Coronæ Borealis. It will be in vain to attempt this latter if all the former, at least ι Bootis, cannot be distinctly perceived to be fairly separated, because it is almost as fine a miniature of ι Bootis as that is of α Geminorum."

This advice is more seasonable to the astrometer than palatable to the astrogazer, who is ever insisting upon arbitrary shifting of objects — on the "now-you-shall-see-what-you-shall-see" scheme. The above-cited hints were originally intended to bear upon the reflecting telescope, yet they contain sensible rules for extreme tests with an achromatic refractor; the which is obviously the more perfect, ready, handy, and durable instrument of the two.

Regarding measures — they are taken up to the present time as well as we can; and the wire-micrometer, rock-crystal prism, Rochon wedges, double image, and divided object-glass, have all and severally been brought into the field: but I was not a little surprised at hearing a recommendation to the Astronomical Society for the restoration of the discarded method of guessing distances between double stars by eye estimations of the apparent diameter of the brighter component—said diameter ever varying in proportion to the power used. Then again we have lately heard much twaddle about polymetric reticles, and the like, by some who make no difference between exquisite measures for physical inquiry and rapid gleanings for zonification. Reticulated diaphragms certainly are useful in mapping stars, and in differentiating them; and, though they can barely be called micrometers, the simplicity of their contrivance renders them desirable objects for many of the borderers of the amateur districts. As the tyro should endeavour to make himself equally strong in mental and mechanical auxiliaries, my object here is merely to caution him not to crave after such humble means, nor overlook them entirely; and should he wish to learn something about their manufacture he cannot do better than consult Dr. Pearson's Practical Astronomy. When he has got one which is furnished with a horizontal wire, or line of any description, let him catch an equatorial star,

and make it run along from one end to the other; when it performs this as it ought to be performed, the other component divisions of the reticular system will consequently become all right.

And now for the re-measurement of certain selected stars, the prefixed Roman numerals being their assigned number in the Bedford Catalogue, or second volume of the Cycle of Celestial Objects:—

VIII. 38 PISCIUM.

Position	235°·9 (*w* 8)	Distance	4″·8 (*w* 4)	Epoch	1837·89
———	237°·8 (*w* 7)	———	4″·5 (*w* 4)	———	1845·91

As the positions recorded in the Cycle, on a seven years' interval, cannot be regarded as conclusive relative to the imputed retrograde orbital march of the components of 38 Piscium, I took the opportunity of a singularly fine state of atmosphere for catching another epoch, compounded of three nights. From the results obtained, a retrocessive motion is out of the question. Under all circumstances I am inclined to apply the relative term "fixity" to the places of this fine object—which are therefore merely optical.

Nor is its proper motion in space so great as Mr. Taylor, of Madras, was led to suppose; the most recent authority—the British Association Catalogue—giving it only, from Mr. Baily's latest comparison:—

In Æ = + 0·005ˢ. In N P D = − 0·11″.

XX. α CASSIOPEÆ.

Position	278°·4 (*w* 3)	Distance	96″·9 (*w* 3)	Epoch	1831·86
———	279°·7 (*w* 9)	———	61″·5 (*w* 6)	———	1851·92

When I examined this wide pair in 1831 I was satisfied with my performance as weighted; especially since H. and S., only ten years before, had found that the *comes* would "scarcely bear the slightest illumination"—a star which to me was so distinct that, but for the glare of its lucida, I might have rated at a ninth magnitude. Hence my request to Mr. Dawes, and his reply

(see *ante*, p. 212). Thinking a bit of a mare's nest had been found, I observed, "the difference of distance is so remarkable, that it must be imputed to instrumental error," meaning, of course, to impugn Sir William Herschel's 56″·17 of fifty years before; but it turned out instead to be a gross blunder of my own, only explicable on the supposition of counting a wrong revolution, with the comb out of the field under the power used. It was not till 1851 that the mistake was pointed out by a friend, to relieve, he said, my "second edition from a typical error." On a trying-back reference, however, it proved to be no fault of the printer, but an unaccountable oversight in the register; indeed my second epoch clearly proved that Sir William had actually made a capital measure, since the alteration of distance is about what is due to the known proper motions of *a* itself (Cycle, II. page 12), in the course of three quarters of a century.

As to the variability of the lucida, I cannot but assign the atmospheric effects of the moon's age to the question; for since the Cycle was printed my views have been considerably shaken on this head (see *ante*, p. 107).

It is curious that a former error, entirely typical, misled Struve as to this pair, and induced him to conclude it was a binary system with an elliptic orbit—the Catalogue of 1782 stating the angle to be 40° 58′ (= 312° 58′ of the present reading) in the north preceding quadrant, instead of 5° 26′ n. p. Here are the latest measured positions, by two zealous collaborateurs:—

POSITION	279°·48	DISTANCE	61″·44	EPOCH	1856·70.—*Jacob.*
———	279°·39	———	62″·41	———	1859·55.—*Pogson.*

XXI. 55 PISCIUM.

POSITION	193°·7 (*w* 6)	DISTANCE	5″·9 (*w* 3)	EPOCH	1833·83
———	194°·3 (*w* 5)	———	7″·0 (*w* 3)	———	1854·07

I mentioned in the Cycle (II. p. 13), that the epochal interval of observations of this beautiful object was too brief for founding conclusions upon; and after a lapse of twenty years I again scrutinised it. Considering the infirmity of the best measures on such a pair, the new results give an inference of fixity;

nor does the primary appear to be affected by proper motion. Equally at rest seem to be the magnitudes and finely contrasted colours of the components; A being noted as deep yellow, and B purplish blue. It must, however, be recollected that it is but little more than thirty years ago when the duplicity of this star was detected by the elder Struve, at Dorpat.

XXIX. η CASSIOPEÆ.

POSITION	92°·0 (*w* 5)	DISTANCE	9″·4 (*w* 6)	EPOCH	1836·81
———	95°·8 (*w* 8)	———	9″·1 (*w* 5)	———	1843·19
———	101°·5 (*w* 6)	———	8″·5 (*w* 5)	———	1846·73
———	108°·3 (*w* 4)	———	7″·7 (*w* 4)	———	1854·17

This very interesting binary system clearly exhibits an increasing orbital velocity, with a rapidly decreasing distance, indicative of an ellipse of considerable ex-centricity. It has been watched by Mr. Fletcher with his smaller but very excellent telescope; and now, armed with mighty means, he will, it is hoped, follow it up to a decided orbit—albeit he may not find its progress so rapid as his recent measures appear to shew. For instance:—

POSITION	106°·63	DISTANCE	8″·166	EPOCH	1851·45
———	110°·18	———	7″·863	———	1853·77

Since I gave the proper motions of this star (Cycle, II. page 19) as severally deduced by Piazzi, Baily, and Argelander, the Rev. Robert Main, comparing its place in the Greenwich Catalogue for 1840 with that inserted in Bessel's admirable collation of Bradley for the epoch 1755, by means of the constants of precession given by Professor Peters in his *Numerus Constans Nutationis, &c.* finds that the resultant spacial movements are—

Æ = + 0·132ˢ. N. P. D. = + 0·49.

XXXII. 36 ANDROMEDÆ.

POSITION	318°·5 (*w* 8)	DISTANCE	1″·1 (*w* 2)	EPOCH	1839·77
———	322°·9 (*w* 9)	———	1″·0 (*w* 4)	———	1843·12
———	335°·8 (*w* 7)	———	1″·3 (*w* 3)	———	1852·83

Though the measures of this golden pair of twin suns are difficult in operation, they can be satisfactorily made. A direct angular variation of 20° in seventeen years under my own eye proves the inference in the Cycle (II. page 21) to be well founded, and leaves its binarity undoubted. The object is certainly widening as to distance; for, though the micrometer does not shew much, the vision keeps them pretty clear of contact; and the same impression strikes two of my correspondents, Mr. Fletcher of Tarn-bank, and the Rev. T. W. Webb of Tretire, near Hereford. Mr. Fletcher's most recent places are—

POSITION 336°·38 DISTANCE 1″·116 EPOCH 1851·93

XXXV. 251 P. O. PISCIUM.

POSITION 301°·8 (*w* 8)	DISTANCE 18″·5 (*w* 5)	EPOCH 1838·03
——— 305°·1 (*w* 6)	——— 18″·8 (*w* 4)	——— 1852·81

The surmise which I made in the Cycle (II. page 23) of the direct orbital motion of this star is abundantly confirmed: and it may safely be entered as one which may be advantageously watched. I have therefore procured its entry on the *agenda* of two or three zealous astronomical friends. The distance seems to remain steady, for an increase in the decimal parts of a second in space cannot be deemed a very positive quantity in such a case as this.

XXXVI. 26 CETI.

POSITION 252°·6 (*w* 9)	DISTANCE 16″·4 (*w* 7)	EPOCH 1833·86
——— 252°·3 (*w* 7)	——— 16″·8 (*w* 5)	——— 1858·10

Although I stated in the Cycle (II. page 23) that the connection between these components was merely optical, yet, as a quarter of a century had lapsed since my observations were made, and a friend had found reason to infer a retrograde motion in angle, on comparing my measures with those of Herschel and South, I again attacked them. As the circumstances - personal, atmospherical, and instrumental—were all favourable to the operations, the last epoch may prove a *quietus*.

XLVII. ζ PISCIUM.

POSITION 63°·8 (w 9)	DISTANCE 23″·4 (w 9)	EPOCH 1839·05
——— 63°·3 (w 8)	——— 23″·1 (w 7)	——— 1846·89
——— 62°·9 (w 8)	——— 22″·8 (w 5)	——— 1857·93

By measures which now extend over seventy-eight years this neat pair still remains without symptoms of orbital motion, and the distance appears unaltered. I have made out nothing as to the supposed variability of ζ, though as to magnitude it is a very bright sixth. From the researches of the Rev. R. Main (see η Cassiopeæ), it seems that both the *comites* of this object have a similar amount of proper motion in space, the value of A's being—

Æ $= + 0{\cdot}008^s$ N. P. D. $= + 0''{\cdot}07$

LI. 42 CETI.

POSITION 332°·8 (w 9)	DISTANCE 1″·2 (w 4)	EPOCH 1834·84
——— 344°·6 (w 8)	——— 1″·3 (w 4)	——— 1857·97

In mentioning the slow direct angular movement of this star (Cycle, II. page 34), I remarked, "but this requires verification," especially as the closeness of the pair renders the measures difficult. But I think the angular motion in orbit is now clearly proved, though the distance, at present, cannot be made much of. The observations of Mr. Dawes confirm this view, and I append the epoch which, from circumstance, he considered his best:—

POSITION 331·88° DISTANCE 1·025″ EPOCH 1842·98

LIX. 100 PISCIUM.

POSITION 78°·9 (w 7)	DISTANCE 15″·9 (w 7)	EPOCH 1833·86
——— 79°·2 (w 8)	——— 15″·6 (w 6)	——— 1852·77

Having stated (Cycle, II. page 38) that by my observations compared with those of the two Herschels and Sir James South, there was presumptive evidence of an angular retrocession = 0°·120 per annum, but that we required

ulterior measures, I directed the telescope to this neat double-star, nineteen years after the conclusion was formed; and from the results then obtained the orbital fixity may be considered as established, nor does the distance seem to be diminishing in the ratio some astrometers have held it to do. The object is an easy one to handle, but it is not often that such coincident measures appear as mine of 1852, and Mr. Fletcher's, with instruments of different apertures, at nearly the same time: here are his places:—

Position 79°·10 Distance 15″·580 Epoch 1852·66.

LX. 123 P. I. PISCIUM.

Position	19°·8 (*w* 7)	Distance	1″·5 (*w* 3)	Epoch	1832·86
———	26°·9 (*w* 9)	———	1″·4 (*w* 4)	———	1843·10
———	26°·3 (*w* 7)	———	1″·5 (*w* 4)	———	1853·91

Though the above measures do not confirm the motion in this beautiful object (Cycle, II. page 39) which I alluded to on consideration of the data then before me, and making every allowance for the difficulties of so close a system, I have no doubt of its binarity. I therefore recommend that it be followed up and well watched by the diligent amateur; and I submit some excellent epochs by Mr. Dawes, which fall between my two first dates:—

Position	24°·00	Distance	1″·711	Epoch	1839·71
———	24°·37	———	1″·542	———	1840·84

By repeated observation under a power keeping them fairly separated during a very fine interval, both the components appear to be a creamy-white tint.

LXXII. γ ARIETIS.

Position	359°·8 (*w* 9)	Distance	8″·8 (*w* 9)	Epoch	1837·93
———	358°·9 (*w* 8)	———	8″·7 (*w* 6)	———	1849·12

These measures indicate no sensible change in this our oldest double-star, for the minute differences are more imputable to the infirmity of measuring than to movements. But there is an appreciable proper motion common to

both stars; and the Rev. Robert Main, by the investigation mentioned under η Cassiopeæ, finds for the lucida this amount:—

$$Æ = + 0{\cdot}002^s \quad \text{N. P. D.} = + 0{\cdot}11''$$

LXXXI. *a* PISCIUM.

Position	333°·4 (*w* 9)	Distance	3″·8 (*w* 9)	Epoch	1838·87
———	331°·4 (*w* 8)	———	3″·5 (*w* 7)	———	1846·92
———	329°·5 (*w* 8)	———	3″·5 (*w* 5)	———	1852·03

In its earlier stages, that is from 1779 to 1821, this splendid pair presented some strange anomalies; but all the later observations combine in shewing a positive retrocession, amply sufficient to testify its binary character, however short they may yet be in affording data to determine the elements of its orbit. My friend Captain Jacob seems to have been much disquieted about the apparent vagaries, yet his own observations in India must tend to bring him to this conclusion, although he thinks the discrepancies only explicable by perturbations—"the perturbing body may either affect *our* system, in which case the motions will be of a parallactic character, or it may act upon the star itself." Since my last measures the following have been handed to me:—

Position	329°·48	Distance	3″·218	Epoch	1853·94.—*Fletcher.*
———	328°·10	———	3″·42	———	1853·99.—*Dawes.*
———	326°·82	———	3″·19	———	1858·05.—*Jacob.*

LXXXII. γ ANDROMEDÆ.

Position	A B	62°·9 (*w* 9)	Distance	10″·6 (*w* 9)	Epoch	1837·80
———	A B	61°·6 (*w* 9)	———	11″·0 (*w* 9)	———	1843·33
———	B C	120°·0 (*w* 1)	———	0″·5 (*w* 1)	———	
———	A B	62°·5 (*w* 9)	———	10″·1 (*w* 9)	———	1852·99
———	B C	115°·0 (*w* 1)	———	0″·5 (*w* 1)	———	

This splendid and high-coloured pair still retains its fixity, but I added another epoch merely to try C again, though I was conscious of the weakness of my means for such a purpose; and, notwithstanding I have so liberally

placed it as 0″·5 apart, it is on a slight guess, for it really appeared to be even closer, and more baffling than when before described (Cycle, II. page 51). Nor is the angle much better than an estimation, the wire having been set to Mr. Dawes's 113° for a trial line; and my wish was rather to find it under than over that fiducial zero. But, though I could not thus accommodate it, I think there can be little doubt of its actually being a beautiful binary system, as stated by the Rev. W. R. Dawes, on the following specific grounds:—

1842·72	Position =	126° 36′.—*Struve.*
1842·83	——— =	125° 48′.—*Dawes.*
1843·33	——— =	120° 0′.—*Smyth.*
1846·81	——— =	113° 19′.—*Dawes.*

Having alluded to the slight proper motion which affects γ, I may here add that the careful investigations of the Rev. Robert Main have brought it to—

$$\text{Æ} = +\ 0{\cdot}001^{s} \quad \text{N. P. D.} = +\ 0''{\cdot}06$$

In March 1857, I received a letter from Sir W. K. Murray, of Ochtertyre, touching the performance of his large telescope, in which he says, "Cloudy unfavourable weather has prevented any observation of the stars you named—on one night only, about a month ago, I enjoyed a fine atmosphere, and had an excellent observation of γ^2 Andromedæ, which was beautifully separated, and the contrast of colours (B yellow, C blue) most distinctly seen. I used a positive eye-piece (720) on this occasion." And for Sir William's further remarks on this celestial beauty, with his practical suggestion, that B appears green under lower powers, is owing to the blending of the blue and yellow tints of the components B and C. (See *ante*, p. 73.)

LXXXIX. ι TRIANGULI.

Position 78°·8 (*w* 9)	Distance 3″·5 (*w* 6)	Epoch 1838·99
——— 78°·5 (*w* 9)	——— 3″·5 (*w* 7)	——— 1857·95

The orbital change suspected in this beautiful object—an idea shaken by my former measures—may now be held as destroyed; and in thereby establishing

fixity, it affords another proof, in conjunction with γ Andromedæ and α Herculis, that highly-coloured stars are not necessarily in rapid motion. Yet this was a dogma which had its adherents a short time ago; and it is colourable.

CXI. γ CETI.

POSITION	288°·8 (*w* 7)	DISTANCE	2″·8 (*w* 4)	EPOCH	1838·92
———	285°·7 (*w* 8)	———	2″·6 (*w* 5)	———	1843·16
———	289°·1 (*w* 7)	———	2″·9 (*w* 5)	———	1855·09

In the Cycle of Celestial Objects (II. page 68), I expressed myself satisfied —on comparing my results with those of Struve, Herschel, and Dawes—that the fixity of this pretty object was established, despite of certain anomalies. Yet, from the communication of a zealous correspondent in 1854—already alluded to under 26 Ceti—I was induced to return to the charge, and obtained such satisfactory results, that my friend took nothing by his motion.

CXXII. 220 P. II. PERSEI.

POSITION	84°·9 (*w* 8)	DISTANCE	12″·4 (*w* 6)	EPOCH	1835·10
———	85°·5 (*w* 9)	———	12″·5 (*w* 7)	———	1843·18

This very neat object was remeasured at Hartwell, not under any doubt of its fixity (Cycle, II. page 74), but to render aid to a beginner. This was the first star in which I tested Piazzi's observations, and by converting his difference of right ascension and declination into angle of position and distance, to obtain a result for the year 1800, which could be registered in comparing the epochs of double-stars. The Cycle (*passim*) gives several unequivocal specimens of the great accuracy of the Palermo Catalogue.

CXXIII. ε ARIETIS.

POSITION	195°·7 (*w* 4)	DISTANCE	0″·8 (*w* 1)	EPOCH	1839·25
———	199°·6 (*w* 7)	———	0″·9 (*w* 2)	———	1843·18
———	200°·1 (*w* 5)	———	1″·0 (*w* 2)	———	1853·08

The assertion which I made in the Cycle (II. page 75) of a change both in angle and distance is confirmed; and my notion—there expressed—of the semblance of its orbit, becomes somewhat countenanced. I hope it will meet with zealous attention among the astrometric corps, for it promises well, being no longer *in contactu*, and therefore within reach of moderate means.

CXXX. τ^4 ERIDANI.

POSITION AB 240°·8 (*w* 1) DISTANCE 150″·0 (*w* 1) EPOCH 1836·90

The measure of this distant star is repeated, because Captain Jacob perceived a close companion to it at Madras. As he rates the magnitude at 10·7, I could hardly have overlooked it, although its place is very unfavourable in this latitude; and, indeed, he remarks that it "would seem to have lately emerged." When I heard of this advent it was too late for me to examine it, but this is the mean of the Captain's results:—

POSITION 287°·04 DISTANCE 5″·47 EPOCH 1857·955

Captain Jacob had previously detected a close *comes* to another star of the Cycle (see ν Scorpii, *post*) at Poonah. The two Struves have worked wonders in this line; and both Dawes and myself have picked up some minute, but not unimportant, specks of light, with our moderate instruments. Yet Mr. Alvan Clark's gleaning Struve's field and there making a discovery of the duplicity of the companion to the oft-examined and measured μ Herculis, with a telescope of only 7¾ inches aperture, of his own making, speaks highly for both eye and instrument: and divers other proofs of his prowess will be found in the Monthly Notices of the Royal Astronomical Society. But there is no end of the findings of such an ardent gazer as this American is: he called upon me recently from Haddenham, where he was on a visit to the Rev. W. R. Dawes, and had there picked up three new delicate double-stars, all in parts of the heavens which have been regularly and unweariedly hunted over, both with the Dorpat and Pulkowa telescopes, and missed. Of these 99 Herculis is a fine object, and one of promise.

CXXXIV. 98 P. III. ERIDANI.

POSITION	231°·8 (w 9)	DISTANCE	5″·9 (w 6)	EPOCH	1834·93
———	235°·9 (w 7)	———	6″·0 (w 4)	———	1845·81

From the discrepancies noted in the Cycle (II. page 82), another epoch of this delicate object was taken; and the results — strengthened by those of Captain Jacob in India soon afterwards—indicate a direct angular movement. It was my intention to have saluted it for further conviction in the apparition of 1858, but other pairs occupied me till it had gone into the sun.

CXXXVI. 40 PERSEI.

POSITION	238°·2 (w 5)	DISTANCE	20″·6 (w 2)	EPOCH	1834·92
———	237°·8 (w 7)	———	20″·1 (w 4)	———	1852·12

As both the identity and early estimations of this delicate double-star have been rather confusedly treated, I took another epoch to learn what it had done in the eighteen years which had lapsed since my measures at Bedford —especially as more than one of my working correspondents seemed to slight it rather unduly. The result, I think, completely establishes its fixity; nor can I find that it has any sensible amount of proper motion.

CXLIII. 30 TAURI.

POSITION	58°·5 (w 8)	DISTANCE	9″·0 (w 5)	EPOCH	1839·90
———	59°·2 (w 9)	———	8″·6 (w 5)	———	1858·10

As Sir John Herschel stated that "no conclusion could be drawn respecting the motion or rest of this star," it became desirable to give the question a *coup de grace*, although I was satisfied as to its relative fixity nearly twenty years before. It is a very neat and delicate object; but I should certainly, on strict comparison at the last epoch, have registered B of the 9th magnitude.

The colours did not seem to be so decidedly strong as they were eighteen years before, for I should have entered them A greenish, B lilac.*

CXLVII. 32 ERIDANI.

POSITION 346°·8 (w 9)	DISTANCE 6″·8 (w 9)	EPOCH 1838·09
——— 346°·5 (w 8)	——— 6″·6 (w 7)	——— 1843·16
——— 347°·0 (w 5)	——— 6″·8 (w 5)	——— 1850·30

The fixity of this fine pair must now be accepted as established, and it affords another example of highly-tinted stars at rest, as noted under γ Andromedæ. I was very well convinced of this in 1843; but, as the object is one of those cited in Sestini's list of coloured stars, I examined it with a darkened field at the last epoch, before making a measurement, under a power of 240, the usual eye-piece for chromatic experiments. The result was that I found for A bright yellow, and for B flushed blue—certainly not white.

CLXV. 80 TAURI.

POSITION 13°·9 (w 5)	DISTANCE 1″·6 (w 3)	EPOCH 1832·16
——— 15°·2 (w 8)	——— 1″·8 (w 4)	——— 1843 11

This difficult though fine double-star exhibits much discordance in the various measures that have appeared; and, though the results appear to justify my expressed opinion as to an orbital movement (Cycle II. page 101), yet such are the twirlings and irradiations attending the operation of the micrometer under illumination, that I cannot consider as yet the fact is proven At present there is the semblance of an increase in the angular velocity, but without

* The star before this in the Cycle, Alcyone, has drawn much attention of late, from M. Mädler's having established it as the centre around which all the visible stars revolve. (See *ante*, page 41.) In my description of it, I submitted a corrected version of the fine allusion to certain stars in Job; but, this not proving to be conclusively satisfactory, I applied to my learned friend S. M. Drach, F.R.A.S., for a literal translation: the following is the reading which he renders:—

Canst thou bind the delightful dainties of Cheemah?
Or the contractions of Ch'scel canst thou open?
Canst thou draw forth Mazzaroth in his season?
Or Ayeesh and her sons canst thou guide?

the diminution of distance which such a binary would require; and Captain Jacob's recent measures at Madras carry away the *comes* in the opposite direction; but he thinks the movements are probably parallatic, or that they may arise from perturbation. Here, however, are his last results :—

Position 7°·73 Distance 1″·56 Epoch 1858·13

All this will be clear enough a few years hence.

CLXVII. 58 PERSEI.

Position B C 30°·3 (*w* 5)	Distance 11·6 (*w* 5)	Epoch 1838·21
——— 29°·8 (*w* 7)	——— 11·8 (*w* 5)	——— 1843·18

This star, as shewn in the Cycle, is assumed as pointer to the pretty pair B and C; the components of which, though small, admit of tolerably easy measurement, whence there has resulted a great accordance both in angle of position and distance. Indeed, except for Sir William Herschel's epoch of 1782·69, the fixity of the pair might be considered as fully established.

CLXX. 2 CAMELOPARDI.

Position 308°·7 (*w* 3)	Distance 1″·7 (*w* 3)	Epoch 1836·28
——— 307°·2 (*w* 6)	——— 1″·5 (*w* 4)	——— 1847·21

This charming but difficult double star can hardly be considered to widen its distance, for the measures at Hartwell in 1847 were taken under the most favouring circumstances; but still, under all the liabilities of such an object, my observations cannot be reckoned conclusive. A small amount of proper motion in A has been detected by the careful scrutiny of the Rev. Robert Main, thus:

Æ = + 0s·002 N. P. D. = + 0″·11

CLXXXVIII. 14 AURIGÆ.

Position A B 224°·5 (*w* 8)	Distance 13″·5 (*w* 6)	Epoch 1832·81
——— 225°·8 (*w* 8)	——— 14″·6 (*w* 5)	——— 1853·11

In the Cycle (II. page 115) I mentioned the suspicion of an orbital change which a comparison of the preceding observations gave rise to, and that my

interval of ten years did not confirm it. Sixteen years after the measures on which I had grounded my opinion I again attacked it; and the result, I think, is sufficient to settle the question as to movement.

In the Cycle there is a slight typographical error in the distance of C in this triple star, which is noticed among the *errata*, namely, instead of 15″·1, it should have been 15″·0, as the trifling sum of weight would indicate.

CLXXXIX. κ LEPORIS.

POSITION 359°·5 (*w* 3)	DISTANCE 3″·7 (*w* 2)	EPOCH 1835·02
——— 357°·9 (*w* 5)	——— 2″·8 (*w* 3)	——— 1851·20

Having been led to attack κ afresh, although I had pronounced upon its fixity, I was surprised to find symptoms, though rather slight ones, of angular retrocession; with a diminution in the distance. It was therefore placed among some objects which I requested Lord Wrottesley to re-examine for me, for the sake of independent comparison. His Lordship's results effectually dissipated all the symptoms of motion in this fine pair, they being:—

POSITION 359°·59′ DISTANCE 3″·250 EPOCH 1857·906

CXC. β ORIONIS.

POSITION 199°·4 (*w* 9)	DISTANCE 9″·5 (*w* 6)	EPOCH 1832·07
——— 199°·6 (*w* 8)	——— 9″·6 (*w* 5)	——— 1850·15

Though there could be no reasonable doubt as to the fixity of this well-observed object, it was re-measured by way of training the eye for encountering disparity, both in magnitude and brilliance, for which this fine star and α Lyræ are so admirably adapted. But it is not a little singular that a pair presenting seeming difficulties should, from 1781 to 1850, keep such accordant places, though examined under a variety of different instruments (Cycle, II. page 116). The proper motion assigned to the lucida by Piazzi, is confirmed by the recent critical investigation of Mr. Main, whose values are:—

$$\text{Æ} = -\,0^{s}{\cdot}001 \quad \text{N. P. D.} = +\,0''{\cdot}02$$

CCV. 118 TAURI.

POSITION 195°·9 (*w* 9)	DISTANCE 5″·0 (*w* 9)	EPOCH 1838·91
——— 197°·4 (*w* 7)	——— 5″·5 (*w* 5)	——— 1858·10

The fixity alluded to in the Cycle (II. page 124), under all probable errors of instrument and lapsed time, must be considered as confirmed by the last epoch. The difference of nearly 5° in angle since Sir W. Herschel first measured it may be a consequence of parallactic action; but it is clear that the distance has remained steady throughout the seventy-five years during which it has been under scrutiny,—varying only from Struve's 4″·89 to Herschel and South's 5″·66, as shewn in their respective catalogues.

CCVIII. β LEPORIS.

POSITION 67°·5 (*w* 1) DISTANCE 210″·0 (*w* 1) EPOCH 1832·00

This star is not introduced here on account of its distant companion, but because A is reported to be close double, the components being rated of 4 and 4½ magnitudes. I have hitherto been unable to prove this, although I have attempted it several times, with powers varying from 240 to 850; and particularly on the fine night of February 4th, 1858, when the glimpse stars in the following field were perfectly distinct. It may be "coming out," and should be carefully watched by those who possess sufficient means.

CCXV. λ ORIONIS.

POSITION 42°·5 (*w* 8)	DISTANCE 4″·6 (*w* 6)	EPOCH 1833·17
——— 43°·0 (*w* 9)	——— 4″·5 (*w* 9)	——— 1843·19
——— 41°·9 (*w* 6)	——— 4″·8 (*w* 4)	——— 1857·11

It will be seen by the weights of the middle of these epochs that I was satisfied of the fixity of this object; but, as a fair opportunity fell in my way of ratifying the conjecture, after a considerable lapse of time, I re-measured the neat double star. The early measures (Cycle, II. page 129) gave an in-

ference of angular retrocession,—but on considering all the causes—instrumental and personal—for slight discordances, I am inclined to repeat my conviction that this pretty pair is without any appreciable motion as yet. On the last occasion the field was darkened after the measures were taken, to observe the colours, when, by two pair of eyes, A was pronounced creamy white, and B pale purple: M. Dembowski states A yellow, B blue, in 1856—*couleurs sûres.*

CCXX. 26 AURIGÆ.

Position	267°·8 (*w* 9)	Distance	12″·3 (*w* 9)	Epoch	1833·09
———	267°·3 (*w* 7)	———	11″·8 (*w* 7)	———	1848·10
———	268°·4 (*w* 7)	———	12″·1 (*w* 5)	———	1853·98

The retrograde motion hinted at in the Cycle (II. page 135), assuredly must be given over, since all the late measures combine against its existence: and to make assurance doubly sure, I requested Lord Wrottesley to measure the pair at a still more recent period. And these are the results which—with his wonted alacrity—he kindly handed to me:—

Position 268°·5 Distance 12·″014 Epoch 1858·228

A is suspected of variability, and at my last gazing at it in 1857 was more like $4\frac{1}{2}$, by comparison with its neighbour, than the fifth magnitude: the observation was, however, inconclusive. The slight proper motion formerly assigned to it, is disappearing under renewed meridional operations.

CCXXIII. ζ ORIONIS.

Position AB	148°·8 (*w* 9)	Distance	2″·5 (*w* 9)	Epoch	1839·19
———	149°·4 (*w* 9)	———	2″·5 (*w* 8)	———	1846·16

I am perfectly satisfied of the fixity of this fine pair, and that Sir William Herschel, from some unknown cause, must have missed B (see Cycle II. page 137); which was afterwards discovered by M. Kunowski. Five years after my re-examination of it, Mr. Fletcher of Tarn Bank found it in—

Position 149·68 Distance 2·672 Epoch 1851·11

and by his appreciation, both the components are yellow. Struve, perplexed with the hue of B, dubbs it *olivaceasubrubicunda*. The researches of the Rev. R. Main (see η Cassiopæa) find proper motions for A, to these values—

$$Æ = -0^{s}{\cdot}002 \quad \text{N.P.D.} = +0''{\cdot}03$$

CCXLV. 8 MONOCEROTIS.

POSITION	23°·8 (*w* 8)	DISTANCE	12″·9 (*w* 8)	EPOCH	1834·19
———	25°·0 (*w* 8)	———	13″·4 (*w* 5)	———	1853·15

By these positions, with an interval of nineteen years, the presumption of a retrograde movement in angle must be dismissed (Cycle, II. page 149); and its fixity is pretty apparent. Notwithstanding Sestini has entered this fine pair as A pale yellow, and B yellowish, they certainly were thought at the last epoch, under powers of 240 and 416, to be nearly as described in the Cycle, namely, A golden yellow, and B flushed blue, or lilac; the object being in better definition than when the tints were reviewed in 1850.

CCL. 11 MONOCEROTIS.

POSITION	AB	130°·3 (*w* 7)	DISTANCE	7″·2 (*w* 5)	EPOCH 1834·02
———	AC	121° 6 (*w* 8)	———	9″·6 (*w* 8)	
———	BC	102°·3 (*w* 7)	———	2″·8 (*w* 4)	
———	AB	131° 6 (*w* 7)	———	7″·4 (*w* 5)	——— 1847·13
———	AC	123°·8 (*w* 6)	———	10″·0 (*w* 4)	
———	BC	102°·5 (*w* 8)	———	2″·6 (*w* 3)	

In the Cycle (II. page 152), following Sir William Herschel, I designated this very beautiful object a "fine triple star;" but it certainly ought to have been registered quadruple; as it appears, on the same quoted page, that it was so called by his son and Sir James South, on their re-examination of it.

The compressed cluster to the north, in the same constellation, No. CCLI. of the Cycle, is entered erroneously on page 152, as being in Æ $6^{h}\ 22^{m}\ 45^{s}$; but it should have been $6^{h}\ 22^{m}\ 25^{s}$, as noted in the list of *Errata;* from which, it is hoped, practical men amend their catalogues by a *coup de plume* over the detected errors, or vexation may follow.

CCLVII. 12 LYNCIS.

Position	AB 149°·5 (*w* 9)	Distance	1″·6 (*w* 9)	Epoch 1839·27
———	AC 305°·6 (*w* 9)	———	8″·9 (*w* 9)	
———	AB 143°·7 (*w* 7)	———	1″·5 (*w* 7)	——— 1852·96
———	AC 306°·3 (*w* 5)	———	9″·0 (*w* 5)	

My last measures of this neat triple star certainly bear out my surmise (Cycle, II. page 156), respecting the probable effect of the retrogradation of A and B, as regards their position with C, their relative position having changed upwards of 32° in 72 years. When the last measures were secured, the field of view was darkened for a re-examination of the colours, when it was decided—by four eyes—that A is of a bright white, B a slightly flushed white, and C a pale blue: in fact, all but the same as they appeared in 1839.

CCLXVIII. 38 GEMINORUM

Position	170°·7 (*w* 9)	Distance	5″·8 (*w* 7)	Epoch	1839·17
———	169°·6 (*w* 8)	———	6″·0 (*w* 5)	———	1848·22
———	170°·2 (*w* 8)	———	5″·6 (*w* 5)	———	1849·19

The amount of annual retrograde motion given in the Cycle (II. page 165), has not been proved by my later measures; still, on a consideration of all the measures I have there cited, we can entertain no doubt of a slow diminution of angle in 70 years. The colours were re-examined as a test because Mr. Dawes, under most favourable circumstances, made A yellow and B blue. On returning to the charge, I find for A pale yellow, and for B purple, as in 1836. Mr. Fletcher also considers A to be yellow, and B purple; and here is the result of his latest set of micrometrical observations—

Position 168°·87 Distance 6″·251 Epoch 1851·89

This fine object may be properly placed on the agenda of the Observatory, as shewing very fair claims—in a retrogade motion of orbit with a probable decrease of distance—to binarity.

CCLXXI. μ CANIS MAJORIS.

Position 342°·9 (*w* 8) Distance 3″·5 (*w* 5) Epoch 1834·15
——— 338°·8 (*w* 5) ——— 3″·0 (*w* 4) ——— 1850·79

This is a very fine object, but one the measures of which are rather anomalous; for though, on the whole, there seems reason to think there are symptoms of orbital retrogradation, a consideration of the infirmities of operating on such a pair will leave the question in doubt. However, it is to be hoped that it will be well watched, and a few years will disclose much. Mr. Fletcher obtained an epoch since my last, to the following effect—

Position 338°·02 Distance 2″·947 Epoch 1852·60

CCLXXIII. ϵ CANIS MAJORIS.

Position 84°·5 (*w* 3) Distance Æ = 24ˢ·1 (*w* 2) Epoch 1834·83

When I marked this distant companion for ϵ, and at other times, the star was so low, and so beset with variable refractions and the abominations of a neighbouring brickfield, that, as I said (Cycle II. page 167), I could not see the nearer *comes*. At the Cape of Good Hope, however, matters were different, ϵ was at a respectable altitude, and my indefatigable friend Mr. Maclear saw this little fellow as a sharp point; the lucida and it being related in magnitude, as Rigel and his companion. By a mean of measurements which he obtained with some difficulty, the places were

Position 160° 25′ Distance 7″·48 Epoch 1850·105

CCLXXIV. 301 P. VI. LYNCIS.

Position 158°·9 (*w* 8) Distance 3″·2 (*w* 6) Epoch 1833·21
——— 159°·4 (*w* 9) ——— 3″·0 (*w* 9) ——— 1843·19

All the discordances which yet appear in the various measures of this neat and delicate double-star, may be assignable to other causes than motion; for on the whole, making certain due though arbitrary allowance, the distance appears

constant, and the angle of position pretty nearly so. Still it bears so little the aspect of an optical object, that it may be recommended for watching.

CCLXXXIII. δ GEMINORUM.

Position 196·8 (*w* 9)	Distance 7″·2 (*w* 6)	Epoch 1838·92
——— 199·8 (*w* 8)	——— 7″·5 (*w* 4)	——— 1847·33

Since the remarks on this delicate double-star were written (Cycle, II. page 173), a direct orbital movement is established, perhaps to the amount of 0°·18 per annum; for, on the whole, my measures indicate a slight change in the angle of position. Under all circumstances, however, more measures, at longer intervals, will be necessary to confirm any notion of physical motion. Mr. Fletcher, who also thinks δ may prove to be a revolver, has established an epoch, since mine was taken, with those values:

Position 200°·67 Distance 7″·321 Epoch 1851·07

CCXCII. α GEMINORUM.

Position AB 254°·9 (*w* 7)	Distance 4″·8 (*w* 4)	Epoch 1838·33
——— AC 162°·2 (*w* 7)	——— 72″·4 (*w* 5)	
——— AB 252°·3 (*w* 9)	——— 4″·9 (*w* 9)	——— 1843·13
——— AC 162°·6 (*w* 7)	——— 73″·0 (*w* 5)	
——— AB 250°·4 (*w* 7)	——— 4″·7 (*w* 4)	——— 1845·82
——— AB 248°·1 (*w* 9)	——— 4″·9 (*w* 5)	——— 1849·17

This neat double star—in some respects the finest in our northern hemisphere—is highly interesting as a binary system (Cycle, II. pages 177-9). After I had obtained the measures in 1845, Mr. Hind forwarded me his orbit for Castor, which, on examination, I found to differ from those alluded to in my description; and that, instead of an *annus magnus* of 240 years which my card-board sectors gave, its period by Hind's Elements is no less than 632 years, and he has since been nearly corroborated by Captain Jacob. "I have recently calculated the elements of Castor and δ Coronæ," said he to me under date of 10th November 1845, "by Sir John Herschel's method, with his

late improvements, for which I am indebted to his kindness. You will no doubt be surprised at the amazing difference between former orbits of Castor and my own; but the observations which have been made since 1835 give quite another form to the ellipse. I only consider my elements at the most a fair approximation. A very few years more must enlighten us considerably." Still there remained the objection which I started, respecting the motions as to angle and distance not being in unison with theory: some observations, however, made after mine were printed, may, I think, convince us that the distance is so on the increase as to countenance the diminution in angle. From Mr. Isaac Fletcher, of Tarn Bank, we have:—

Position	AB 247°·97	Distance	5″·075	Epoch	1852·12
——	AC 163°·13	——	72°·144		
——	AB 245°·66	——	5°·309	——	1857·28

In addition to the physical connexional changes, the proper motions in space must be considered, for by the Rev. Mr. Main's process, mentioned under η Cassiopæa (*ante*, page 218), the values are found to amount to

$$Æ = -0^s{\cdot}013 \quad \text{N.P.D.} = +0''{\cdot}08$$

CCXCVIII. α CANIS MINORIS.

Position 85°·0 (*w* 1) Distance 145″·0 (*w* 1) Epoch 1833·81

As Procyon had been one of Piazzi's principal parallax stars (Cyclo, II. page 183), I gave it as a companion the nearest distinct star in the field, in case it should be again in requisition for that purpose; and it was then shining in mid-distance between the lucida and an 8½ magnitude yellow star in the following field about 5½ minutes off. In this position it was seen both by Mr. I. Fletcher in Cumberland, and the Rev. T. W. Webb in Herefordshire, even after it had been declared "missing" in America. (See the Astronomical Monthly Notices, vol. xiii.) There is something strange and unaccountable in the matter: at first I thought the absentee might be a planetoid, but its having been seen for several years forbids the assumption, while similar

difficulties attend the idea of variability. At present, all I can do in the mystery is to place the fact on record, and leave time to unravel it; in aid of which I add an extract of Mr. Fletcher's letter to the Royal Astronomical Society, dated May 5th, 1853 (see Monthly Notices, vol. xiii. page 222).

In 1848, Mr. Bond, of the Cambridge U.S. Observatory, announced that the small star was "missing." In 1850 I saw and measured the position of the companion with ease, and estimated it as of the ninth magnitude. My measures gave this result:—

1850·17 POSITION 84° 19′

During the spring of this year I have looked most carefully for this small star with my 6-foot achromatic, but I have never obtained a trace of its existence.

At the beginning of last year, the weather being singularly favourable, I instituted a search for the absentee's re-appearance; but, though I teazed the spot under powers 340 to 850, I returned *re infectâ*. Still, being much struck with evanescent glimpses of light near the place, which I could not make positive, I requested Mr. Dawes to apply his eight-inch object-glass upon it; and this energetic astronomer, though suffering under a severe neuralgic head-ache, muffled himself up and started off to the Observatory at Hopefield, which is at some distance from his then temporary residence. By a letter written on the following morning—9th February, 1858, he informed me that he "could detect nothing worthy of being called a star" in the position of the missing *comes;* but that, "while poking about," he picked up a very small companion to Procyon, which he had never heard of before, very delicate indeed, in these rather estimated than measured places—

POSITION 285° ± DISTANCE 48″ ±

While I was pondering over this phenomenon, my friend Mr. E. J. Cooper, of Markree Castle, sent me the fourth volume of his Ecliptic Stars, wherein he shows a very extraordinary table, namely—"A list of stars given in the Markree Catalogue, now MISSING": and this gentleman has since informed me that he has no doubt that above half of them will prove to be asteroids, since they were all well observed. This communication adds largely to what

I advanced—after enumerating Herschel's insulated stars—of stellar disappearances (Cycle, I. pages 274-5): and there is a remarkable paper in point by M. Chacornac, of the Imperial Observatory at Paris, "On several Stars which have disappeared from his Ecliptical Charts." This the reader will find in the Royal Astronomical Society's Monthly Notices, vol. xv.

Piazzi erred less in the proper motions of this star, than he did in its parallax, for from the scrutiny and comparison of the Rev. Robert Main, we have

$$\text{Æ} = -0^{s}{\cdot}048 \quad \text{N.P.D.} = +\ 1''{\cdot}08$$

It will be recollected that this special movement, compared with its want of uniformity with almost the same amount in α Canis Majoris, led Bessel to hazard the bold speculation that Sirius and Procyon are binary systems, in which one only of the two stars is visible, because only one is luminous; the variability observed arising from their relative orbital motion about their common centre of gravity—light being no property of mass, nor the existence of numberless visible stars any proof against the existence of numberless invisible ones. At least the irregularities which Bessel detected, could only be explained by Sirius and Procyon moving in orbits under the influence of central forces, and consequently round another star, which, being invisible, must be an opaque and non-luminous one. Time will decide.

CCCIV. κ GEMINORUM.

Position	231°·9 (w 6)	Distance	6″·0 (w 4)	Epoch	1838·98
———	232°·3 (w 5)	———	5″·8 (w 3)	———	1851·21

From the increase of angle between Mr. Dawes's epoch and mine, as seen in the Cycle (II. page 186), I was led to attack this elegant but delicate object again, and obtained the above results. But, on firmly gazing with time and power, the small star is too conspicuous to be shining by reflected light, as suggested by Sir John Herschel,—at least, so it struck me. The second epoch of Mr. Dawes yielded these results:—

Position 232°·67 Distance 6″·183 Epoch 1841·201

CCCXV. ζ CANCRI.

Position	AB	5°·2 (w 8)	Distance	1″·3 (w 3)	Epoch	1839·32
———	AC	148°·2 (w 9)	———	5″·1 (w 6)		
———	AB	355°·1 (w 8)	———	1″·2 (w 5)	———	1843·11
———	AC	147°·2 (w 9)	———	5″·0 (w 6)		
———	AB	345°·5 (w 6)	———	1″·0 (w 3)	———	1847·28
———	AC	147°·4 (w 5)	———	5″·0 (w 3)		
———	AB	322°·7 (w 6)	———	0″·9 (w 3)	———	1853·17
———	AC	144°·1 (w 8)	———	4″·8 (w 5)		

Since I mentioned some of the probable elements of this wonderful ternary system (Cycle, II. page 194), additional observations have brought the orbit of A and B so nearly circular that the time of its periodic revolution cannot be much short of a century; while the retrocessive movement of A and C may be noted as —0·47 per annum; a sensible change, but one which will require ages for the performance of its revolution. Moreover the mutual action of the three bodies must necessarily exercise a disturbing force.

Mr. Fletcher took some measures of this interesting object nearly at the same time with my last epoch, and these are the results:—

Position	AC	143°·68	Distance	4″·842	Epoch	1852·49
———	AB	321°·06	———	1″·100	———	1853·30

CCCXX. φ² CANCRI.

Position	212°·5 (w 9)	Distance	4″·8 (w 9)	Epoch	1833·25
———	213°·9 (w 9)	———	4″·8 (w 9)	———	1843·19
———	214°·2 (w 7)	———	5″·0 (w 5)	———	1857·22

I was well satisfied of the fixity of the fine and close double star (Cycle, II. page 196), but that a doubt was thrown in by a friend's measurement, which implied a retrocession in orbit. My last epoch, however, may be held conclusive as to a retrograde movement; for, difficult as the measures may be, if the fixity be not adhered to, the comparisons would certainly indicate a slow direct motion in orbit. Fifty years hence it will be clear enough. By the Baron Dembowski, both components are equal in magnitude.

CCCXXI. υ^1 CANCRI.

Position	38°·6 (w 8)	Distance	5″·7 (w 6)	Epoch	1837·26
———	40°·1 (w 9)	———	5″·8 (w 8)	———	1843·18

The retrograde motion of —0°·514 (Cycle II. page 196), which was inferred from a comparison of my first epoch with that of Sir William Herschel, is not only negatived by my measures at Hartwell, but they also afford a slight evidence that, if there be any orbital movement, it must be direct. The colours at the last epoch, in a darkened field with power 240, were A creamy white, and B pale blue: the Cycle magnitudes are from Piazzi.

CCCXXVI. 108 P. VIII. HYDRÆ.

Position	24° 9 (w 9)	Distance	10″·5 (w 9)	Epoch	1839·06
———	25°·7 (w 8)	———	10″·4 (w 6)	———	1849·13

This neat object, which is designated 18 Hydræ by some, must now be pronounced in a state of fixity; the differences in angle being assignable to possible instrumental and personal errors of observation. Having received Sestini's list of the colours of double stars, and finding that he called these two components yellow, I took some pains in re-examining them in order to fix a point; and the aid of Mrs. Smyth and Dr. Lee was called in. The result was, that A must be termed full yellow, and B a flushed white or pale pink: at the same time the assigned magnitude of B was confirmed.

CCCXXXVIII. ϵ HYDRÆ.

Position	191°·1 (w 9)	Distance	3″·5 (w 6)	Epoch	1839·23
———	203°·2 (w 9)	———	3″·6 (w 8)	———	1843·14

On comparing all the places of this lovely object, I think a direct orbital movement may be traced, amounting to about 0°·65 per annum; but the supposed decrease of distance is not yet confirmed. Mr. Fletcher's observations, taken at Tarn Bank nearly ten years after my last epoch, give

Position 208°·52 Distance 3″·578 Epoch 1852 96

Mr. Main, by the rigorous comparison which I mentioned under η Cassiopæa, found that the proper motions assignable to A were

$$Æ = -0^s{\cdot}013 \quad \text{N.P.D.} = +0''{\cdot}04$$

CCCXLIII. 17 HYDRÆ.

POSITION 357°·8 (*w* 9)	DISTANCE 4″·5 (*w* 9)	EPOCH 1838·12
——— 358°·5 (*w* 8)	——— 4″·5 (*w* 5)	——— 1849·21

Among the revelations of astronomy, that of double stars is one of the most curious and interesting, especially in branching into its two divisions—optical and physical (Cycle, I. page 293). Now, the fine object before us being one of those pairs of which the proximity is accidental, I was well-assured of its relative fixity (Cycle, II. page 208); but it was measured afresh, on the ground that B, being at a remoter distance than A, might show a proper motion in the latter. But the problem still requires time, and on the whole is, perhaps, a case for the large heliometers.

CCCLVII. ω LEONIS.

POSITION 355°·0 (*w* 2)	DISTANCE *elongated* (*w* 1)	EPOCH 1839·33
——— 193°·0 (*w* 3)	——— 0″·3 (*w* 1)	——— 1843·14

It will be seen in the Cycle, (II. page 216) how little reliance is to be placed in my estimations, or rather guesses, respecting this difficult and egg-shaped but exquisitely cuneated object. Indeed it was only mis-shapen to me by glimpses, nor would I even make an affidavit that I had a firm view of the elongation. But its being one of Struve's *pervicinæ* has not precluded it from being recognized as a binary system; and M. Mädler has ably calculated the elements of an orbit, having a period of 82·533 years.

CCCLXXVI. γ LEONIS.

POSITION 106°·0 (*w* 9)	DISTANCE 2″·6 (*w* 9)	EPOCH 1839·23
——— 107°·2 (*w* 9)	——— 2″·8 (*w* 9)	——— 1843·18

The colours of the components of this very beautiful pair were re-examined,

and without referring to the Cycle, noted A flushed yellow, and B a similar tint but paler. Mr. Fletcher obtained an epoch ten years after my last, at the Tarn Bank observatory, of which the values are

POSITION 108°·43 DISTANCE 3″·003 EPOCH 1853·21

which in round terms shows a change of angle amounting to about 25° in 71 years; thus proving the slow direct movement to which I alluded in the Cycle (II. page 228). But the duration of the *annus magnus*, as there stated, may perhaps, on further consideration, be much reduced.

While here, those who will not refer to a table of *Errata* may be reminded, that on the preceding page 227, line 5, N.P.D. 77^h 15^m 12^s, ought to have been 77° 15′ 12″. The typical error, however, will be obvious enough.

CCCLXXIX. 67 P. X. LEONIS.

POSITION	64°·8 (*w* 5)	DISTANCE	3″·0 (*w* 3)	EPOCH	1831·18
———	65°·3 (*w* 7)	———	3″·5 (*w* 5)	———	1843·16
———	67°·5 (*w* 6)	———	3″·5 (*w* 5)	———	1853·22

These last results have induced me to suspend the opinion which I gave in the Cycle (II. page 230), that this "beautiful but delicate object is only optical;" for, though the measures are somewhat impaired by the diffusion of light and moulding of the components while under operation, they are sufficiently strong to indicate a progressive movement in orbit, which, however slow it may appear, should be looked after by the future astrometer.

CCCLXXXI. 49 LEONIS.

POSITION	158°·1 (*w* 5)	DISTANCE	2″·5 (*w* 5)	EPOCH	1838·37
———	159°·0 (*w* 7)	———	2″·8 (*w* 3)	———	1855·29

In the Cycle (II. page 231) it will be seen, that from the great difference between Struve's angle and mine, in an interval of only 7½ years, I thought a retrograde angular motion was indicated. This, however, was expressed with caution, and left questionable. But, though the object is extremely delicate, and at times not at all of easy measurement, I am pretty well satisfied with my last operations; and therefore pronounce for its fixity.

CCCXCI. 54 LEONIS.

Position	102°·7 (w 8)	Distance	6″·2 (w 8)	Epoch	1839·33
———	103°·3 (w 8)	———	6″·4 (w 5)	———	1855·29

The sentence I passed in the Cycle (II. page 236), that little change can have taken place in this pretty pair in 58 years, is now confirmed by observations made after an interval of over 74 years. Indeed I was pretty well satisfied of its fixity, on making all allowances; but, finding the accurate Mr. Dawes had obtained a position of 105°·18 in the winter of 1846, I again placed it on my working agenda for another attack.

CCCCVI. ξ URSÆ MAJORIS.

Position	156°·9 (w 8)	Distance	2″·0 (w 5)	Epoch	1839·23
———	143°·2 (w 9)	———	2″·3 (w 6)	———	1843·16
———	136°·1 (w 7)	———	2″·8 (w 4)	———	1845·75
———	132°·8 (w 8)	———	3″·0 (w 5)	———	1849·18
———	123°·5 (w 7)	———	2‴·9 (w 4)	———	1851·31

This very interesting binary system has been well watched, and its motions most ably discussed, though substantially it remains as described in the Cycle (pages 246, 247). In that description I alluded to the great discordances in the value of its orbital movement,—and they are not yet removed by observation, insomuch that the aid of perturbation and proper motion is called in. Sir John Herschel, reasoning on such apparent anomalies, observes that the two halves of the orbits seem to belong to different ellipses. By graphically projecting the places of the various epochs, we are led to an ellipse of which the gravest elements are 95°·5 for the node, 0·4350 for the excentricity, and about 60 years for the period; which is in fair agreement with my former conclusion, as mentioned in the Cycle (II. page 247): and, on comparison, the computed places agree tolerably with the observed values. From Mr. Fletcher's last measures, obligingly made for my use, we have

Position 111°·25 Distance 2″·992 Epoch 1857·40

CCCCXI. ι LEONIS.

Position 87°·7 (*w* 6)	Distance 2″·4 (*w* 3)	Epoch 1839·32
——— 86°·0 (*w* 8)	——— 2″·5 (*w* 4)	——— 1843·38
——— 81°·3 (*w* 7)	——— 2″·5 (*w* 4)	——— 1853·29

In treating of this beautiful pair in the Cycle (II. page 250), I mentioned that I scrupled not to designate it a binary system; and from observations in various quarters, in addition to my own, the binarity is now fully proven. The measures of Mr. Dawes, to which I there alluded, gave

Position 85°·27 Distance 2″·628 Epoch 1843·26

and since my last epoch, Mr. Fletcher has handed me the following—

Position 81°·43′ Distance 2″·092 Epoch 1855·27

On the whole, the series of operations on components so unequal may be said to indicate a retrograde movement in angle of about 0°·5 per annum, instead of 0°·834 as concluded by the elder Struve. It is possible that the distance may be on the increase, though certainly not as shown by the measures, but in that to the *senses* it seems easier that it was. In the Cycle I gave three values for the proper motions, but an astronomer thought they were still more sensible. That question may be considered as disposed of by the investigation of the Rev. R. Main, who gives the quantity—

Æ = + 0s·007 N.P.D. + 0″·07

The colours were again scrutinized in 1857, under a darkened field, when A was pronounced to be a fine yellow, and B a decided blue.

CCCCXVI. 57 URSÆ MAJORIS.

Position 9°·9 (*w* 8)	Distance 5″·9 (*w* 5)	Epoch 1835·42
——— 8°·3 (*w* 7)	——— 5″·5 (*w* 4)	——— 1846·38

This fine double star was re-measured rather to test how B—suspected of variability—would bear illumination again, than for the sake of another epoch of an object so apparently in a state of fixity. The result, however, under all allowances, certainly gives indication of a probable retrograde motion

in orbit, and it may therefore be recommended for watching. As to B, by my rule-of-thumb comparison, it again appeared as a bold 9th-magnitude violet-tinted star; yet it has been recorded as a red 10th. Can this result from change? Mr. Hind thinks that alterations of brilliance are accompanied by alternations of hue: that severable variable stars increase blue, are yellow after maximum, flash red in decreasing, and, at their minimum brightness are surrounded with a kind of fog. This interesting suggestion awaits the fiat of confirmation, but it is, notwithstanding, pregnant with meaning.

CCCCXX. 17 CRATERIS.

POSITION 207°·8 (*w* 3) DISTANCE 10″·1 (*w* 2) EPOCH 1833·21
——— 259°·8 (*w* 5) ——— 9″·3 (*w* 3) ——— 1846·38

This neat double star was also watched, like 57 Ursæ Majoris, on account of the suspected variability of B; it having been registered, by various observers, from 5½ to the 8th magnitude, while I have adhered, agreably to the rule I have mentioned, to the 7th, as recorded by Piazzi. However had I, upon systematic comparison, found it different, the fact, of course, would have been duly noticed. In our latitude the star is low down, and liable to be involved in variable refractions; but Sir John Herschel, who measured this pair at the Cape of Good Hope, with an advantageous altitude, registered it as being of the 6th degree of brightness in 1835.

As there were appearances of a direct orbital motion, I requested Lord Wrottesley to give me an epoch with his equatoreal for further comparison, and of the consequent observations, these are the values—

POSITION 211°·39 DISTANCE 8″·819 MAGS. 5 and 5½ EPOCH 1857·292

whereby the increase of angle with a diminution of distance is confirmed; and the reason for the non-influence of the proper motions of A which I advanced in the Cycle (II. page 255) is borne out. The value of this movement in space was there given for A only; but, as the relative situations of A and B had not altered, so that a physical connection was obvious, Mr. Main treated the components as one mass; and, comparing its place in the Greenwich

Catalogue of 2156 stars with other meridional registers, found the proper motion in Æ = $-0^s{\cdot}005$, and in north polar distance = $-0{\cdot}11$.

CCCCXXXIII. 2 COMÆ BERENICIS.

POSITION 239°·9 (w 9)	DISTANCE 3″·6 (w 3)	EPOCH 1839·37
——— 238°·5 (w 8)	——— 3″·6 (w 5)	——— 1849·35
——— 240°·2 (w 9)	——— 3″·3 (w 7)	——— 1855·28

Notwithstanding my satisfaction with the results of the epoch of 1849, and the opinion expressed thereon in the Cycle (II. page 261), I was tempted by two fine nights in succession to return to this admirable object. The circumstances were equally favourable with those recorded in 1849; and a very careful review of the whole measures taken between 1832 and 1855, compared with those of other astrometers from 1782, has convinced me of the optical nature and consequent fixity of this very neat pair.

CCCCLVI. γ VIRGINIS.

POSITION 169°·9 DISTANCE 3″·8 EPOCH 1858·39

We have now arrived at a very extraordinary and crucially important astronomical fact, namely, a well-ascertained instance of one sun actually revolving round another sun, under similar dynamics with those of our Solar System— to which, until lately, the attention of astronomers was almost exclusively directed. Indeed, before the advent of Wright and Michell, the sidereal heavens were mostly regarded as a mere assemblage of fixed stars, so unchanging in their appearance and unvaried in their character as to possess very little interest. Since then, the speculations and labours of such master-minds as those of Kant, Lambert, and Sir William Herschel, have fully demonstrated the fallacy of such ideas, and enlarged the apparent boundaries of human knowledge to an unlimited extent.

As I shall give the whole "story" of this most interesting binary star in the eighth chapter of this book, I need only here place my last and farewell results before the persevering reader, together with the sum of the measures taken in the twelve well-spent evenings on which they depend:—

DATE. t.	Position. $\angle$	Wght. w.	w. $(\angle - 170^\circ)$	w. $(t. - 1858)$	Observs.	Distance. Δ	Wght. w.	w. $(\Delta - 4'')$	Observs.
1858.	° ′		′			″			
Feb. 4, ·10	169 „ 43	40	− 680	+ 400	8	3·44	24	− 1344	6
—— 8, ·11	169 „ 36	36	− 864	396	7	4·01	16	+ 16	4
—— 18, ·14	170 „ 31	46	+ 1426	644	9	4·01	26	+ 26	6
May 6, ·35	170 „ 53	40	+ 2120	1400	8	3·55	19	− 855	6
—— 7, ·35	169 „ 56	44	− 176	1540	7	3·90	30	− 300	6
—— 8, ·36	170 „ 37	41	+ 1517	1476	7	3·56	24	− 1056	6
—— 10. ·36	170 „ 23	42	+ 966	1512	7	3·97	29	− 87	6
—— 11, ·36	169 „ 15	45	− 2025	1620	7	3·97	24	− 72	6
June 13, ·46	169 „ 30	20	− 600	920	6	3·79	18	− 378	6
—— 14, ·46	168 „ 57	35	− 2205	1610	7	4·11	24	+ 264	6
—— 15, ·46	168 „ 54	48	− 3168	2208	8	4·15	31	+ 465	8
—— 18, ·47	170 „ 9	40	+ 360	1880	8	3·43	25	− 1425	6
398		−268	− 9718	+ 15606	89		−193	− 5517	72
		+209	+ 6389				+ 97	+ 771	

CONCLUSION.

Sum of $(\angle\ s.w.)$, ∴ mean of POSITION = 169°, 54′·31
Sum of $(\Delta\ w.)$, ∴ mean of DISTANCE = 3″·7937
Sum of $(w.\ t.)$, ∴ mean of EPOCH = 1858·392

Corresponding Angles and Distances forwarded expressly for comparison with the above Measures, from the following Observatories, namely:—

Observatory.	Position.	Distance.	Epoch.	Observer.
GREENWICH	168° „ 30′	3″·847	1858·48	*R. Main.*
HADDENHAM	168 „ 47	3″·682	1858·45	*W. R. Dawes.*
TARN BANK	170 „ 01	3″·567	1858·387	*I. Fletcher.*
WROTTESLEY	170 „ 42	3″·401	1858·476	*F. Morton.*

CCCCLXII. 35 COMÆ BERENICIS.

Position AB	30°·0 (w. 1)	Distance	1″·0 (w. 1)	Epoch	1834·38
——— AC	126°·5 (w. 8)	———	28″·8 (w. 6)		
——— AB	42°·0 (w. 2)	———	1″·5 (w. 1)	———	1843·32

When my first measures were taken at Bedford, there were anticipations that this delicate object would turn out to be a ternary system; but A and C are decidedly mere optical objects, as stated in the Cycle (II. page 285). A and B have, however, apparently a direct progression of about 0°·6 per annum; a conclusion which calls for confirmation. In 1853 in merely gazing with a negative eye-piece magnifying 240 times, in a darkened field, the colours came up bright and clear, A being pale yellow, B lilac, and C a full blue.

CCCCLXVI. 12 CANUM VENATICORUM.*

Position	227°·0 (w. 9)	Distance	19″·8 (w. 9)	Epoch	1837·39
———	226°·7 (w. 9)	———	20″·2 (w. 7)	———	1853·60

Here is additional proof of the relative fixity of this fine double star, during a period of 73 years; and the proper motions, as established by the investigation of the Rev. Robert Main, (see η Cassiopæa) are thus valued—

Æ = — 0s·023, and N. P. D. — 0″·06

This object was again scrutinized in 1855 as to its contested colours; when it was declared by some good eyes that A is of a pale reddish white, and B a lilac tint (see *post*, Chap. VII).

CCCCLXXI 42 COMÆ BERENICIS.

Position	10°·0 (w. 2)	Distance	*elongated* (w. 1)	Epoch	1839·41
———	5°·0 (w. 1)	———	0″·3 (w. 1)	———	1842·50
———	*round* (w. 5)	———	*round* (w. 5)	———	1843·32

I certainly thought that this impracticable object—one of Struve's high class *viciniissimæ*—was opening, although my estimations were but vague

* In my mention of the Court parasites seeing this star brighter on Charles II. returning from exile (Cycle, II. page 288) I should have added that, if John Ogilby is to be believed, Charles's birth was also attended by a star! "May that great God who sent a star to wait on your nativity," is John's ejaculation in the dedication of his ponderous Homer. Pretty well for the Master of his Majesty's Revels!

guesses; still the image gave an impression of an egg-shaped form, especially at the epoch of 1842. But in the following year, in the finest weather, and under the utmost coaxing, I was compelled to give up all idea of the elongation, and record it as a single star with my limited means.

CCCCLXXX. ζ URSÆ MAJORIS.

Position	147°·4 (*w* 9)	Distance	14″·4 (*w* 9)	Epoch	1839·32
———	148°·1 (*w* 9)	———	14″·2 (*w* 9)	———	1854·72

Although I was well-assured of the fixity of this brilliant double star, as expressed in the Cycle of Celestial Objects, (II. page 298, 299,) I was led after an interval of fifteen years to re-examine it; and the results which were then obtained, fully corroborate the opinion I had advanced.

This splendid object, Mizar, was the principal of those selected by Mr. G. P. Bond, the American Astronomer, to test the powers of photography upon; and a specimen of his success which he sent over to Greenwich in 1857, representing the whole field between Mizar and Alcor, was so much to the admiration of our Astronomer-Royal, that he wrote to me in terms of excitement as to the achievement, and intimating that the present hum-drum scheme of measurement would shortly receive its *viaticum*. It is indeed truly wonderful, and to a certain degree probably efficient; but I am not altogether so sanguine in my anticipations, since some grave difficulties seem to stop the way in advancing to perfection. Yet the mean of Mr. Bond's pictures, as compared with the mean of all my observations, give the places of the star and its companion with singular and unexpected precision; being thus—

Position	147°·8	Distance	14″·49	*By photograph.*
———	147°·7	———	14″·40	*By wire micrometer.*

It appears that no fewer than eighty-six photographs of Mizar were taken, and an achromatic reading-microscope was employed, when "the bisections of the images," says Mr. Bond, "are made with an exactness quite surprising, considering their indefinite outline and comparatively large diameter." But

he also says—"The real difficulty, perhaps insurmountable, which now prevents a most extensive application of photography to astronomy, is the deficient sensitiveness of the processes in use. Unless photographs of stars as low at least as the eighth magnitude can be obtained, its use must be restricted to comparatively few double stars. Should, however, this impediment be overcome, and photographic impressions be obtained from stars between the sixth and eleventh magnitudes, as has already been done for those between the first and the fifth, the extension given to our present means of observation would be an advance in the science of stellar astronomy, of which it would scarcely be possible to exaggerate the importance."*

CCCCLXXXIV. 51 M. CANUM VENATICORUM.

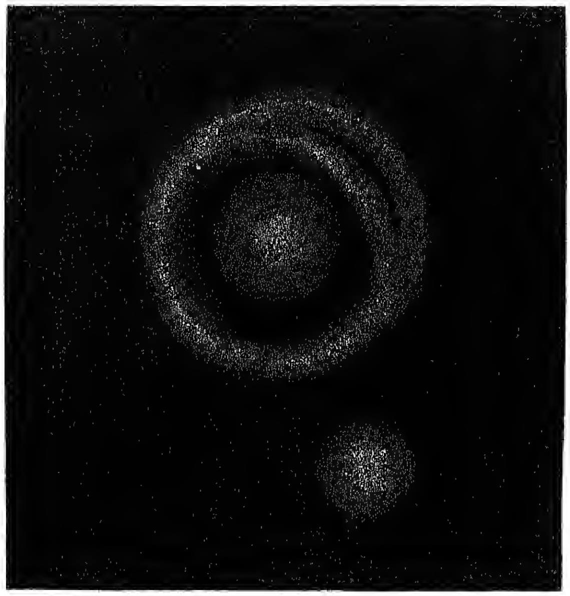
MEAN EPOCH OF OBSERVATION { 1836·69 *At Bedford.*
1857·54 *At Hartwell.* }

Thou shalt lead us on to triumphs, yet to mortal power unknown,
Realms which angels only visit, shall yield homage at thy throne.
*   *   *   *   *
Far almost as thought can venture, which Immensity scarce bars,
Deep in that unfathom'd ocean, all whose isles are heavens of stars.

DR. ROBINSON, ON THE ROSSE TELESCOPE.

When Messier discovered this wonderful nebula sixty-four years before my first acquaintance with it, he merely noted it as a faint double nebula. Sir William Herschel described it as a bright round nebula surrounded by a halo or glory, and attended by a companion; while his son thought it bore a real physical resemblance and strong analogy of structure to our own system: and this was his representation of it, as published in his elaborate Catalogue of 1830; but here the shading—as regards black and white—is inverted, and the whole adjusted to a *sf, nf, np, and sp,* direction:—

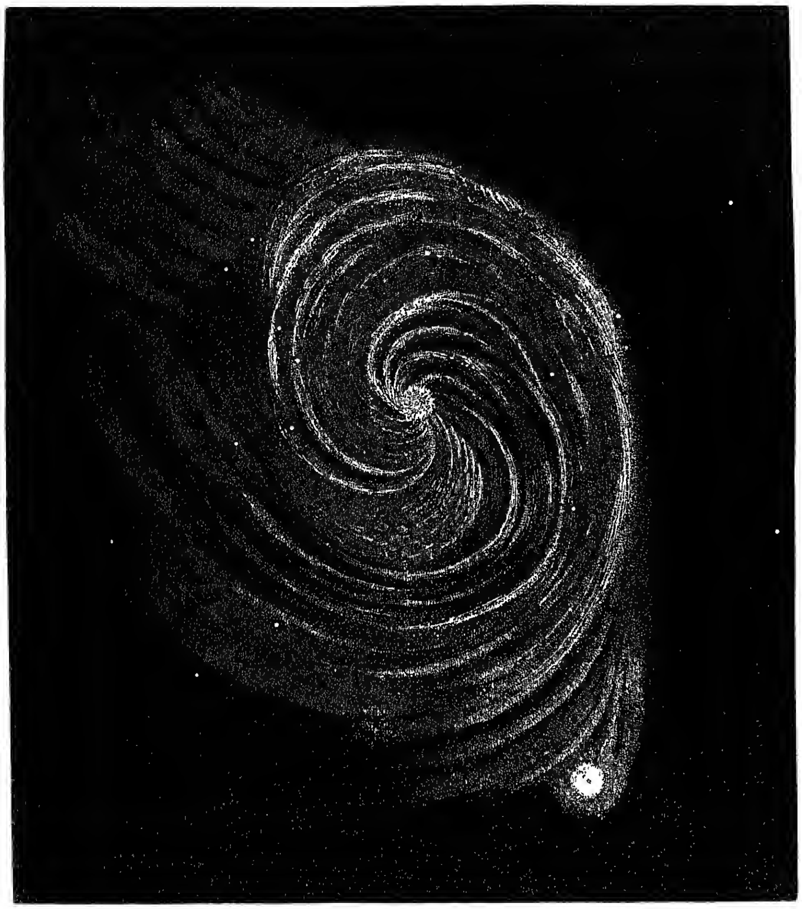
* To my traditional legends of Ursa Major (Cycle, II. page 238), it should have been added that King Arthur, the renowned hero of the Mabinogion, typified the Great Bear, as his name—*Arth* bear, and *Uthyr* wonderful—implies in the Welsh language: and the constellation visibly describing a circle in the north polar region of the sky, may possibly have been the true origin of the son of Pendragon's famous Round Table, the earliest institution of a military order of knighthood.



My own expression, after much close gazing, was (Cycle, II. page 302), "a pair of lucid white nebulæ, each with an apparent nucleus, with their nebulosities running into each other, as if under the influence of a condensing power." But under all the ability of my instrument, charged from 63 to 240 times, I could not even raise a spectrum of the above figure; a diffused gleamy light only, as marked on my sketch, being occasionally caught on intense watching. Somewhat disappointed with this performance of the equatoreal, I wrote afterwards to Mr. Frederick Morton, Lord Wrottesley's assistant, to ask what his lordship's telescope—of nearly two inches more aperture than that which I plied—would reveal in the case. This gentleman's reply was in perfect accordance with the statement I had already given, saying—"there is certainly a diffused nebulosity perceptible between the two nuclei, and it appears brighter or more condensed at a certain point (which may be a faint nebulous star) nearly mid-way between them. Between this point and the northern nucleus, there is a patch of sky apparently much blacker than the rest of the field." This latter peculiarity is no doubt owing to the effect of the bright patch giving intensity to the darkness immediately around; however, this is all that I could make of 51 Messier at Bedford—

Yet really the whole was nearly a "sealed book" till the spring of 1845, when the enormous command of light possessed by the magnificent telescopes of Lord Rosse enabled him to detect certain mystic convolutions in the nebulæ, which hitherto had remained undetected by human eye. I speak of those wondrous bodies which exhibit polar and lateral foci under a spiral arrangement—seemingly vortices of remote star-streams, bent into curves by orderly yet apparent irregularities of impulse. It may be true that spiral arrangement is not a demonstration of spiral motion, but we cannot resist the speculation of compound movements indicating forces of which we are entirely ignorant. In such cases, where appearances seem beyond the laws of which we have any knowledge, we can only bow, and refer their structure to the fiat of OMNIPOTENCE. To enable others to think also, we will here submit a very faithful though inverted copy of the view of this nebula—that is, I have placed it white on a dark ground instead of black on a white field, as more consonant to what we really see. And this is its strangely novel appearance, with representations of which Lord Rosse absolutely entranced the intellectual world; carrying out Dr. Robinson's happy prediction as to the consequences attendant on his lordship's mighty telescope—

> Many a bright and starry wonder, hid since time its course began,
> Many a secret of creation shall be now disclos'd to man.



The Earl of Rosse, in making this revelation, observes—"Much however as the discovery of these strange forms may be calculated to excite our curiosity, and to awaken an intense desire to learn something of the laws which give order to these wonderful systems, as yet, I think, we have no fair ground even for plausible conjecture; and, as observations have accumulated, the subject has become, to my mind at least, more mysterious and more unapproachable." And again, on the apparent *concordia discors,* his lordship says:—

We thus observe that with each successive increase of optical power the structure has become more complicated, and more unlike any thing which we could picture to ourselves as the result of any form of dynamical law of which we find a counterpart in our system. The connection of the companion with the greater nebula, of which there is not the least doubt, and in the way represented

in the sketch, adds, as it appears to me, if possible, to the difficulty of forming any hypothesis That such a system should exist without internal movement, seems to be in the highest degree improbable; we may possibly aid our conceptions, by coupling with the idea of motion that of a resisting medium; but we cannot regard such a system, in any way, as a case of mere statical equilibrium.

In a word, the discovery of SPIRAL NEBULÆ, the most extraordinary and unexpected class of objects which modern research has yet disclosed in stellar astronomy, is wholly due to the noble Earl: and their forms are so entirely removed from all analogy with the phenomena presented either in the bodies of the solar system, or the whirls of Encke's comet, in the star-curves I found in 35 Messier (Cycle, II. page 144), or in the stellar strata of Padre Secchi, that notions as to their physical condition are merely guess-work. The enigma is another unequivocal mark of the illimitable power of the SUPREME CREATOR!*

For excellent accounts of other wonderful revelations of Lord Rosse's telescopes, the reader is referred to his Lordship's description in the *Philosophical Transactions* for 1850; and also to the lucid communications of Dr. T. R. Robinson, of the Armagh observatory, to the Royal Irish Academy.

CCCCLXXXVI. 127 P. XIII. VIRGINIS.

POSITION 31°·0 (w 8)	DISTANCE 1″.7 (w 4)	EPOCH 1838·48
——— 37°·9 (w 8)	——— 1″·7 (w 6)	——— 1842·52
——— 51°·7 (w 6)	——— 2″·0 (w 5)	——— 1852·38

This star was remeasured because it evinced so great a change in angle with but little in distance; and it seems to be still under the same conditions, the which are strongly indicative of a binary system with a circular orbit (Cycle, II. page 304). The pair were measured nearly at the middle epoch above given, by the Rev. W. R. Dawes, and the coincidence was remarkable—

POSITION 37°·40 DISTANCE 1″·637 EPOCH 1842·39

* The spirality of nebulæ has been strangely suggested to be owing to scratches on the metallic mirror used: but I hope this notion is abandoned, since it is utterly untenable either upon optical or mechanical grounds.

CCCCLXXXVIII. Prec. 3 M. CAN. VENATICORUM.

Position 191°·5 (w 3)	Distance 1″·0 (w 1)	Epoch 1835·48
——— 195°·0 (w 2)	——— 1″·0 (w 1)	——— 1851·37

This little beauty precedes that brilliant globular cluster 3 Messier, and, having been detected by me at Slough, in the Herschelian 20-foot reflector, is of course an object of interest. I mentioned in the Cycle, (II. page 305), that it was decidedly elongated to Mr. Dawes and myself when we examined it together in 1843; and this gentleman has since obtained some very fair measures of it with his Mertz 8½-foot refractor, of 6⅓ inches aperture, at his observatory of Camden Lodge, near Cranbrook in Kent, thus:—

Position 196°·54 Distance 0″8 Epoch 1848·42

by which I was led to conclude a change of angle; but from the difficulty of ascertaining this with my means, the question is still an open one.

DVI. α BOOTIS.

Position 49°·3 (w 2) Difference Æ = 15s·1 (w 1) Epoch 1835 47

This noble and insulated standard Greenwich star is re-introduced, because its large proper motions have been investigated on two occasions since I mentioned their amount as then settled by Piazzi, Baily, and Argelander (Cycle, II. page 315): and as the movement may prove to be of the highest moment in physical inquiry, I will here recapitulate all the ascertained values—

Æ = —1″·17	Declination — 1″·96	*Piazzi.*
—— —1″·11	——— — 1″·98	*Baily's first.*
—— —1″·18	——— — 1″·96	*Argelander.*
—— —1″·18	——— — 1″·97	*Mädler.*
—— —1″·18	——— — 1″.93	*Main.*
—— —1″·17	——— — 1″·96	*Baily's second.*

Since my description was published, the little star *infra Arcturum*, on a bearing of 182°·2, was enlisted by Johnson for a parallax-pointer to Arcturus, because from 1690 to 1855—on comparing the standard authorities, no proper

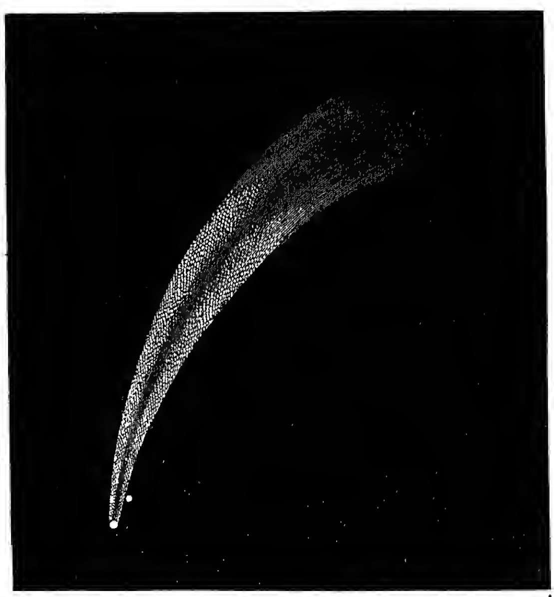
motion can be detected. In his investigation of this question, he obtained for Arcturus the error of spacial movement in distance = $0''\cdot0384 \pm 0''\cdot040$; hence the quantity already assigned must be very near the true value. The resultant parallax by Johnson, and that of Peters, may be thus compared :—

$0''\cdot127 \pm 0''\cdot073$.—*Peters.*
$0''\cdot1377 \pm 0''\cdot052$.—*Johnson.*

or, assuming the proper motion given by Main as exact for the year of observation, and rejecting the very dubious correction for temperature, we have

$0''\cdot169 \pm 0''\cdot044$.—*Johnson.*

Regarding the brightness of Arcturus, although it always maintains its place among the first rates, it is suspected of a tendency to variability. Indeed the charge amounts to even more than suspicion, for Mr. Fletcher has noted it alternately brighter and less bright than Capella, a star so favourably posited for direct comparison. It was, however, in full splendour on the 5th of October 1858, when, to the delight of millions of human eyes, it was run over by Donati's beautiful comet, the head of which was instantly reduced to "second best" in brilliance by the star, despite a new veil thousands of miles in thickness. This was its appearance at about 5^h 35^m P.M.



In a letter which I received from Mr. Pogson, he regretted that certain observatory cares, and an unusual invasion of sight-hunting bores, prevented him from making those cometary observations with the excellent Oxford instruments which he otherwise wished to have done; but he adds:—

One observation of light-intensity, however, we did obtain when Arcturus was so near as to be in the same field of view with the comet, and if you will make any record of it, I can assure you that it is a most trustworthy comparison, carefully made, by an excellent method.

The comet, seen through the undiminished half of the heliometer object-glass, appeared of equal intensity with Arcturus seen through the other half diminished to 0·95 inch. Arcturus was therefore 62·3 times as intense as the comet, or, adopting Argelander's ratio of 2·512, the difference of magnitude was 4·5.

On September 30th, at 7^h 17^m mean time, Mr. Luff of Oxford, whose honest worth—well-known to yourself—gives weight to all he states, measured the length of Donati's tail with a sextant; he found it to be 22°.

Now had the nucleus of Donati (Comet V. 1858) passed but about 20′ further to the north than it did that evening, and gone right over Arcturus, what a world of physical questions as to phasis, density, solidity, and permeability to solar energy might have been cleared off! As it is, philosophers must wait until some other comet shall pass over a brilliant star.

DXVIII. ζ BOOTIS.

Position	128°·6 (*w* 8)	Distance	1″·3 (*w* 6)	Epoch	1838·45
———	127°·3 (*w* 9)	———	1″·2 (*w* 6)	———	1842·43
———	128°·2 (*w* 6)	———	1″·0 (*w* 4)	———	1852·38

Another attack on this close, and in some respects inconvenient double-star, confirms my former opinion that no orbital variation has occurred since 1796, notwithstanding apparent discrepancies; nor does there, on weighing the whole, appear to be any difference in the distance (Cycle, II. page 324). The trifling anomalies exhibited in a period of 56 years, since Sir William Herschel first measured it, must be in the instrumental manipulation, for no proper motion can be traced in declination, and only + 0^s·002 in right ascension.

Nor has the strange alteration of brightness alluded to in the Cycle, been

confirmed; though the elder Struve expressly says—"nihil est dubium quin differentia luminis in his stellis sit variabilis."

DXX. ϵ BOOTIS.

Position 321°·2 (*w* 7)	Distance 2″·9 (*w* 5)	Epoch 1838·68
——— 322°·1 (*w* 8)	——— 2″·8 (*w* 5)	——— 1848·54

This beautiful star, the *pulcherrima* of Struve, still maintains an apparent fixity by my measures, although so much evidence has been adduced for its binarity: and I am more inclined to assign the anomalies which appear in the records, to the closeness, the brightness, and the oblique position of the components, than to orbital changes, or proper motions through space (Cycle, II. page 325), the last of which are now found to be only

$$Æ = -0^s{\cdot}005 \quad \text{N. P. D.} = -0''{\cdot}01$$

I cannot but adhere, therefore, to the opinion I expressed, that progression is not proved; and another instance is afforded, as with γ Andromedæ and α Herculis, that high colours in stars are not symbolical of motion.

Mr. Sollitt, of Hull, informed me that he thought he saw A of this object double, in a reflecting telescope of his own making; but I could give no encouragement to the notion. However I sent the case to Mr. Dawes, for the benefit of his instrument, who, on the 20th of November, 1855, returned this answer—"In a letter to myself some months ago, Mr. Sollitt preferred a charge of duplicity against Mrs. ϵ Bootis; whereupon I put her on trial, calling as witnesses two capital object-glasses of 7½ inches and 7¾ inches diameter respectively; and no browbeating on my part could induce them to declare that there was any duplicity about her character. Having full confidence in my witnesses, that they aver the truth as far as their optical power extends, I am bound to acquit the said ϵ of the charge; at least until the evidence against her is proved to be stronger and more trustworthy than that in her favour." This must be regarded as a definitive sentence.

DXXII. ξ BOOTIS.

POSITION 324°·8 (w 9)	DISTANCE 7″·1 (w 9)	EPOCH 1839·61
——— 322°·9 (w 9)	——— 6″·9 (w 9)	——— 1842·42
——— 316°·8 (w 9)	——— 6″·5 (w 5)	——— 1852·38

These last measures form an additional evidence in proof of the orbital retrogression of this fine pair of stars (Cycle, II. page 328), no less than 22° having passed under my own eye; the angular velocity being about 0°·74 per annum. As the hues of this object have been questioned, as well as its binarity, it was specially gazed at in 1857, by two pair of eyes, in a darkened field of view, without reference to the Cycle notation; and the thereby unbiassed result was A, deep yellow, B, flushed purple.

DXXIII. 39 BOOTIS.

POSITION 44°·7 (w 9)	DISTANCE 3″·8 (w 6)	EPOCH 1839·00
——— 45°·3 (w 7)	——— 3″·5 (w 5)	——— 1847·61

The slow retrograde motion assigned generally to this fine double-star, since it was discovered by Sir William Herschel in 1783 (Cycle, II. page 329), is certainly not borne out by my micrometrical measures: and even some change which may be traced in nearly three quarters of a century, is assignable to proper motion, which at the last comparison was valued at

$$\text{Æ} = -0^{s}{\cdot}011, \text{ N.P.D.} = -0''{\cdot}05$$

Mr. Dawes and myself differ more on the colours of this pair, than is usual with us; he having noted both as pale yellow, while I make A white, and B pale lilac—the magnitudes, in 1847, scarcely differing.

DXXIX. 44 BOOTIS.

POSITION 235°·3 (w 9)	DISTANCE 3″·5 (w 7)	EPOCH 1839·62
——— 235°·9 (w 9)	——— 3″·7 (w 7)	——— 1842·58
——— 236°·2 (w 7)	——— 4″·1 (w 5)	——— 1847·45

This fine object is a mystery; for, although after weighing all the data with attention I feel unable to say anything decisive as to angular progress, there can be no doubt of its increase of distance, an opening which stamps its physical character. But the evidence as to its binarity remains still nearly the same with that given in the Cycle (vol. II. p. 334). Since my last measures, Mr. Isaac Fletcher obtained the following places:—

Position 237°·95 Distance 4″·268 Epoch 1851·47

On looking at this pair in 1856, in a dark field, A appeared to be pale yellow, and B dusky; which is nearly Struve's *sub-flava* and *sub-cærulea.*

DXLI. η CORONÆ BOREALIS.

Position	120°·1 (*w* 6)	Distance	0″·5 (*w* 1)	Epoch	1839·67
———	151°·3 (*w* 5)	———	0″·5 (*w* 1)	———	1842·58
———	188°·5 (*w* 3)	———	0″·3 (*w* 1)	———	1846·69
———	246°·8 (*w* 3)	———	0″·5 (*w* 1)	———	1852·43

The handling of this fine binary system is, from the brightness and proximity of its components, extremely difficult; and indeed the distances are mere guesses, to which my lowest weights are applied, but perhaps O ought to have been introduced into my scale. By transient glimpses the elongation was pretty fair, but never notched, and therefore the angles merit more consideration than the distances. Since the Cycle was published, orbits have been computed by M.M. Mädler and Villarceau, but the principal elements do not remotely differ from those which appear therein (II. page 341)—the ex-centricity still remaining less than that of any other computed binary system, except ζ Cancri. In addition to what is said as to the proper motions of η on the same page, Mr. Main has valued its movement in north polar distance at +0″·19, by rigorous comparison with standard meridional catalogues.

DXLIII. μ^2 BOOTIS.

Position	310°·6 (*w* 7)	Distance	0″·9 (*w* 2)	Epoch	1839·32
———	306°·1 (*w* 6)	———	0″·8 (*w* 2)	———	1842·52
———	255°·0 (*w* 3)	———	0″·5 (*w* ?)	———	1853·60

The relative situation of this highly interesting binary system with μ^1 Bootis remains nearly the same, both in position and distance, as given for the year 1832, in the Cycle (II. page 342):* but *inter se* the retrograde movement in orbit is still further confirmed, and with accelerating velocity. Hence it follows that the distance is diminishing, it having been observed by Herschel and South in 1823 as $= 1''\cdot65$, and in 1853 I hardly considered it to $= 0''\cdot5$: my last was rather a guess than an estimate.

It will be seen in the Cycle (II. page 343), that I considered the period of this system to be a shaky quantity. Since that volume was printed, I have been favoured by Mr. John Russell Hind with a letter from Allsop's Place, 24 September, 1845, in which, among other scientific matter, he says——"I beg to send you a set of elements for the binary system μ^2 Bootis (Piazzi, XV. 74), which I have just computed on Sir John Herschel's beautiful method. All your observations are included in the calculations, but I have not yet had leisure to compare them with my elements, as I intend doing:—

Perihelion Passage . . .	τ =	1849·41 A.D.
Mean annual motion . . .	μ = —	36′·102
Angle between nodes and apsides .	λ =	102°·23′
Position of node	Ω =	116°·54′
Ex-centricity	e =	0·8746
Inclination of orbit . . .	γ =	58°·11′
Semi-axis major	a =	3″·874
Period of revolution . . .	P =	598·3 years.

DXLVII. δ SERPENTIS.

Position	197°·3 (w 8)	Distance	2″·7 (w 8)	Epoch	1838·38
———	196°·2 (w 9)	———	2″·8 (w 8)	———	1842·35
———	196°·5 (w 7)	———	3″·0 (w 5)	———	1851·32

* In the note to that page, I have called the *Khazzan* the chief of the Synagogue: but Mr. S. M. Drach, who has readily aided my Hebrew researches (*see ante, page* 227), assures me that the office is that of reader, or prelector—the head Rabbi being always the chief of the Synagogue.

I cannot altogether think that the binarity of this beautiful double-star is incontestibly proven, although the angle for 1782·99 was 227° 12′, thus indicating a retrocession of about 31° in 70 years; the distance remaining steadily the same during the while. But in the last 30 years, that is, from Herschel and South's epoch to the present time, the angular motion has scarcely exceeded 3°; which is a smaller amount than what was alluded to in the Cycle (II. page 345). Here we must observe and wait.

DXLIX. ζ CORONÆ BOREALIS.

Position	300°·9 (*w* 9)	Distance	6″·5 (*w* 9)	Epoch	1839·50
———	301°·2 (*w* 9)	———	6″·1 (*w* 9)	———	1842·57

This very beautiful double-star must be classed as an optical object, my Hartwell operations proving so satisfactory (Cycle, II. page 347). To be sure there is a difference in angle of 6° in about 63 years, from the time when Sir William Herschel first registered it; but all the later measures are coincident in affording reason to question whether any change has taken place in either position or distance. ζ has a slight positive proper motion in space.

DLI. γ CORONÆ BOREALIS.

Position	A.B. 225°·0 (*w* 1)	Distance	0″·3 (*w* 1)	Epoch	1839·69
———	*round* (*w* 8)	———	*round* (*w* 8)	———	1842·58
———	295°·0 (*w* 2)	———	0″·5 (*w* 1)	———	1848·37

This delicate and difficult double star, truly one of the *vicinissimæ* of Struve, at my last observation was surely wedged, or egg-shaped, with the companion "coming out again," but both position and distance are rather estimated guesses than measures—except in the instance of 1842.

I mentioned in the Cycle (II. page 348) my conviction that A was then much brighter than the sixth magnitude assigned to it in the Palermo Catalogue. The latter may probably have been a mere typographical blunder, yet it has been

followed in other Catalogues with an injurious effect; for Lord Wrottesley, purposely observing stars down to the fifth magnitude inclusive, omitted γ Coronæ, because he found it rated a sixth. Now, since it seemed to me as bright as β, I applied to the Astronomer Royal for his opinion, and in a letter from Greenwich, 18th August, 1857, he thus expresses himself:—

On reference to Littrow's publication of Piazzi's originals it appears that the magnitude of γ Coronæ (as of other stars) is given only once in the year, although there are several observations. This necessarily leaves great opening for error.

The magnitudes set down are as follows:—1792—6; 1796—4; 1809—5. In the Catalogue of 1803 it is 4·5, and in that of 1814—6. It seems most likely that the 6 was designedly omitted in forming the Catalogue of 1803.

Be this as it may, I must confess that it has generally struck me as being of a good fourth degree of brightness; and there can be no doubt of its being a case where error must be suspected rather than variability.

DLVIII. 51 LIBRÆ *seu* ξ SCORPII.

Position	A B	13°·3 (*w* 8)	Distance	1″·1 (*w* 5)	Epoch 1838·60
———	A C	74°·2 (*w* 8)	———	7″·2 (*w* 6)	
———	A B	23°·5 (*w* 8)	———	1″·2 (*w* 5)	——— 1842·56
———	A B	24°·9 (*w* 7)	———	1″·0 (*w* 3)	——— 1846·49
———	A C	68°·1 (*w* 6)	———	7″·0 (*w* 4)	

This fine triple-star offers, at present, a complicated scheme, since A and B exhibit a direct orbital motion, while A and C are retrograding, as mentioned in the Cycle (II. page 353). Before my last operations I placed the mean motion of A and B at + 1°·85; but Sir John Herschel has concluded that by his own measures from 1830 to 1835, he found an angular velocity of + 1°·69. Assuming its orbit to be in all probability nearly circular, I had assigned about a century for its *annus magnus;* but, though the observations were too barren for conclusive elements, there now are such symptoms of elliptical elongation that the period thereby becomes extremely uncertain. Captain Jacob, W. S. has recently (July 1858) drawn attention to the subject, saying—

I beg to call the attention of astronomers possessing powerful telescopes to the present condition of the close pair of the ternary star 51 Libræ. The early measures of this pair were con-

sidered by Admiral Smyth (see Cycle, vol. II. p. 352) to indicate a circular orbit; but the stars, which have latterly been gradually approaching, have within the last two years closed up so rapidly as to be in the early part of the current year quite beyond the power of my instrument, the distance being estimated as not exceeding 0″·4, while only a rough guess could be made at the angle under the most favourable circumstances. I have not yet computed an orbit, though the data are, I believe, sufficient for doing so approximately. The apparent orbit must be highly elongated, and the period somewhere about fifty-two years.

DLX. κ^1 HERCULIS.

Position	9°·7 (*w* 9)	Distance	31″·4 (*w* 9)	Epoch	1835·45
———	9°·2 (*w* 7)	———	30″·6 (*w* 5)	———	1857·38

Though I was satisfied, on comparison of my Bedford measures with those of Herschel, South, and Struve, that the deductions respecting the decrease in distance derived from Flamsteed's observations must be in error, I took another set after a lapse of twenty-two years; thus carrying the interval of discussion over 154 years. The question I think is now at rest; neither angle nor distance have altered beyond probable errors of measurement, and the comparative fixity of the components is therefore established.

DLXI. ν SCORPII.

Position	A B 338°·5 (*w* 6)	Distance	40″·0 (*w* 4)	Epoch	1831·50
———	A B 336°·8 (*w* 7)	———	40″·8 (*w* 5)	———	1851·38
———	B C 45°·0 (*w* 2)	———	1″·5 (*w* 1)		

This neat object was discovered to be triple by Captain Jacob at Poonah, in 1847, who saw that B had a companion of the eighth magnitude; his instrument being a 5-foot telescope, charged only with a power of 152. As Sir John Herschel had not seen it with his 20-foot reflector at Feldhausen, where it was in his sweep 722, the Captain observes "probably C has recently emerged." In consequence of its southern declination, and the nuisance of the neighbouring brick-field, the new *comes* was difficult to view firmly, or to measure fairly; it was therefore carefully estimated both in angle and distance under the bar micrometer. The inconvenience of ν's place was still more felt at Lord

Wrottesley's Observatory, it being further to the north, where however it was seen plainly double, and treated as below mentioned.

Regarding A and B, I think we may be satisfied as to their fixity, for by some measures recently made for comparison with mine by Mr. Frederick Morton, at the Wrottesley Observatory, we have—

A and $\frac{B+C}{2}$ POSITION 337° 40′ DISTANCE 41″·055 EPOCH 1857·39

Differing very little from the places settled in 1779, by Sir William Herschel. The colours, from lowness of the star's place, and perhaps the different powers employed, were not easy to define with precision, my impression in 1851 being A yellowish white, B pale lilac; while Lord Wrottesley's notation for 1857 was A yellow, and B blueish. C is of the non-descript tint called dusky.

DLXII. 49 SERPENTIS.

POSITION 318°·1 (*w* 9)	DISTANCE 3″·3 (*w* 9)	EPOCH 1839·29
——— 323°·0 (*w* 7)	——— 3″·2 (*w* 8)	——— 1854·58

Here we find the orbital retrocession between the years 1783 and 1854, continues to an amount not widely different to that already given in the Cycle (II. page 355); though latterly the velocity appears to have slackened, especially when I refer to my own observations only. There is, however, sufficient evidence to indicate binarity; and a period of 600 years is countenanced.

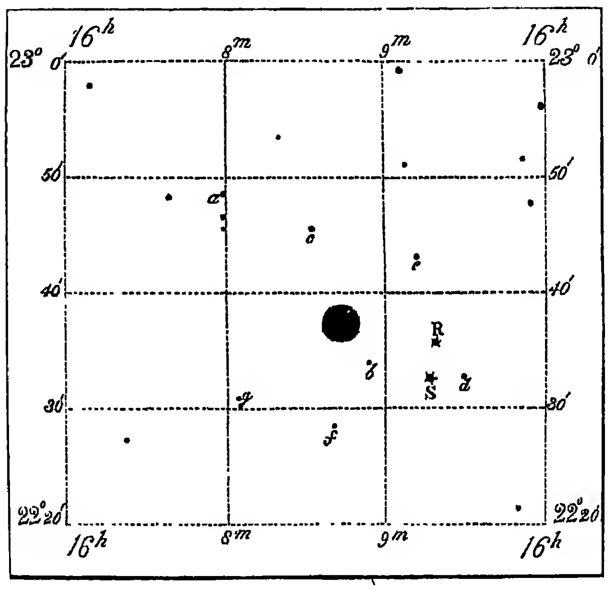
On darkening the field of view to re-examine the colours of the components at the last epoch, a very minute star hove in sight, which I had not noticed before: it is near the line formed between B and a distant ninth-magnitude one in the north following quadrant, which, small as it is, appears strongly blue.

DLXIV. 80 M. SCORPII.

MEAN EPOCHS OF OBSERVATION { 1837·36 *At Bedford.*
1858·47 *At Hartwell.*

Those who have taken the trouble to look into the Cycle of Celestial Objects (II. page 357), will have observed a wood-cut in which the globular

cluster called 80 Messier is seen, with its attendant stars in the following part of the field. The whole forms the remarkable object already described in this volume (*see ante*, pages 104-5); and which I will thank the reader just to run his eye over again, in order to start fair with the following tabulated data. But first look at the illustrative configuration of the cluster, the two variable stars R and S and the stars of comparison; one of the latter, namely *f*, being strongly suspected of a tendency to change—the mean positions of the nebula and its companions being reduced to the epoch 1860.



After having had a convincing proof of the changes which R and S had undergone since I first assigned them a magnitude, I requested Mr. Pogson to supply me with whatever particulars he could furnish in the lapsed time between my epochs, as he had been some years attending to the phenomena of Variable Stars. In a reply, dated 5th July, 1859, he said:—

I beg leave to submit to your notice the latest and most trustworthy elements of variation of your pair of stars, near the nebula 80 Messier, in Scorpio, discovered to be variable by M. Chacornac. They are the best I have been able to deduce from a discussion of the scanty data available for that purpose. R, the south-following of the pair, was seen as a 12th-magnitude last year, but was lost in the twilight soon after, long before its due time of maximum. S, the north-preceding one,

on the contrary, which appears to vary more rapidly, was not seen last June, when R was visible; and only as a 12th-magnitude at its re-appearance this spring.

But for your records in 1837 and 1839, the elements would be mere guesses, whereas I now attach some weight to them as tolerable *first approximations.* In using your two dates, however, it is well to name that I took the liberty of altering your estimations, just as I should my own under similar circumstances. My comparison star, *d*, forms the north-following angle of your small acute-angled triangle; and as on each occasion this star occurs in your configurations, I made your estimation differential instead of absolute, and classed the variables '*about the* 10*th magnitude—same as d.*' Here are the data:—

OBSERVATIONS OF R SCORPII COLLECTED AND REDUCED.

DATE.	Co-ordinates of Curve.		Observed			REMARKS.
	Day of Century.	Standard Magnitude.	By	At	Power.	
1837, April 30	13634	10	Smyth	Bedford	93	Noted in the configuration of 80 Messier; also comparison star *d* and S Scorpii; all as 11 mags.
1839, May 7	14371	10	„	„	118	Noted in the configuration of 80 Messier; also star *d* and S Scorpii; but all as 8 mags.
1853, July 1	19540	9	Chacornac	Paris	...	Discovery of variability.
„ „ 6	19545	9	„	„		
1854, May 19	19862	Under 14	„	„	...	Invisible.
1855, April 14	20192	9	„	„		
„ May 18	20226	11	„	„	..	Similar to comparison star *e*.
„ June 4	20243	12	„	„	...	Inferior to comparison star *e*.
1856, April 29	20573	Under 14	„	„	...	Invisible.
„ June 25	20630	Under 14	„	„	...	„
1858, June 18	21353	12	Pogson	Oxford	96	
„ „ 30	21365	12	„	„		
„ July 17	21382	Under 11·5	„	„	...	Invisible; fine, but near the horizon.
1859, March 31	21639	Under 12·7	„	Hartwell	118	Invisible in a fine sky, but low altitude.
„ April 4	21643	Under 13	„	„	...	„ „ „ „
„ May 6	21675	Under 12·5	„	„	...	„ Very fine.
„ June 23	21723	Under 13	„	„	52	„
„ „ 30	21730	Under 13	„	„	118	„ Splendidly fine.

Only five maxima are available for this star, but much more satisfactory and trustworthy than those for S Scorpii. They extend over twelve periods, and are as follows:—

1837 April 30	10 mag.	—*Smyth.*
1839 May 7	10 „	—*Smyth.*
1853 July 4	9 „	—*Chacornac.*
1855 April 14	9 „	—*Chacornac.*
1858 June 24	12 „	—*Pogson.*

Treating by least squares we obtain—

Epoch = 1858 Sep. 8·5

Period —— 645·6 days.

Computing from these elements we have—

Maxima.	Residual Errors.
1837 June 22·7	53·7
1839 March 30·3	37·7
1853 May 20·5	44·5
1855 Feb. 25·2	47·8
1858 Sep. 8·5	76·5

The sum of the squares being 14422.

Agreeably to Chacornac's observations in 1855 a change of 3 magnitudes occupied 51 days, and thereby, reducing each date to an assumed average maximum brightness of 9th magnitude, we get—

1837 May 17	Hence
1839 April 20	Epoch
1853 July 4	1859 Oct. 3·7
1855 April 14	Period
1858 August 20	648·14 days.

Residual errors 32·0—22·9—26·7—27·6—44·7.

The sum of the squares being only 5020.

Hence this hypothesis is preferable.

From these discussions—which represent the observations hitherto made with very considerable accuracy—we are able to arrive at a general conclusion, namely, that with far greater certainty than can be yet pronounced for S Scorpii, R usually reaches the 9th magnitude at maximum; the greatest observed fluctuation amounts only to about one-fifteenth of the period, and the most probable elements of variation may be thus assumed:

Epoch = 1858 October 4. Period = 648 days.

Such inquiries into the differing periods of brightness, are deeply important in connecting the profound results which perseverance has obtained from the pursuit of practical Astronomy.

In the preceding extracts from Mr. Pogson's form of entry, the column "*Day of Century*" is a very convenient addition, as it facilitates the formation of equations by showing the number of days elapsed between different maxima at sight, without the tedious delay occasioned by counting up the unequally long months, or the risk of forgetting to make allowance for the bissextiles. A table for converting ordinary dates into what may be termed the "*time-co-ordinate*" is speedily arranged, and of use in planetary, cometary, and many other kinds of computation, besides that for which it is here employed.

Now for a similar treatment of the other Variable star, S, which is the north-preceding of this extraordinary pair—

OBSERVATIONS OF S SCORPII COLLECTED AND REDUCED.

DATE.	Co-ordinates of Curve.		Observed			REMARKS
	Day of Century.	Standard Magnitude.	By	At	Power.	
1837 April 30	13034	10	Smyth	Bedford	93	Noted in the configuration of 80 Messier; also R Scorpii and comparison star *d* as 11 mags.
1839 May 7	14371	10	,,	,,	118	Ditto; but S Scorp., R Scorp., and star *d* all noted 8 mag.
1853 July 6	19545	Under 12	Chacornac	Paris	...	Not seen in charting.
1854 May 19	19862	9·5	,,	,,	...	First discovery of variability.
1855 April 14	20192	10·3	,,	,,	...	Similar to comparison star *d*.
,, May 18	20226	13	,,	,,		
,, June 4	20243	Under 14	,,	,,	...	Completely gone.
1856 April 29	20573	11·5	,,	,,		
,, June 25	20630	Under 14	,,	,,	...	Completely gone.
1858 June 18	21353	Under 13	Pogson	Oxford	96	Invisible.
,, ,, 30	21365	Under 13	,,	,,	,,	,,
,, July. 17	21382	Under 11·5	,,	,,	65	,, Low, but brilliantly fine.
1859 March 31	21639	12·2	,,	Hartwell	118	Low altitude, but very fine.
,, April 4	21643	12·0	,,	,,	,,	,, ,, ,, ,,
,, May 6	21675	Under 12·5	,,	,,	,,	Invisible, though very fine.
,, June 23	21723	Under 13	,,	,,	52	,,
,, ,, 30	21730	Under 13	,,	,,	118	,, Splendidly fine.

Now as this star was visible in May, 1854, and again in April, 1855, it is evident its changes must be completed in much less time than those of its neighbour R Scorpii. From simple inspection it appears to add one more to the evident majority of year periods:—as if something tellurian were the analogical key to the explanation of the Variable Stars.

It was not considered worth while to apply relative weights to the equally uncertain maxima derived either from this or the preceding table; though it may be remarked that a judicious assignment of weights is of the utmost importance in these, as in all other combinations of observations.

The maxima deducible from all records up to this year are six.

1837 April 30	—10 mag.	—*Smyth.*
1839 May 7	—10 „	—*Smyth.*
1854 May 19	—9·5	—*Chacornac.*
1855 April 14	—10·3	—*Chacornac.*
1856 April 29	—11·5	—*Chacornac.*
1859 April 2	—12·1	—*Pogson.*

These dates extend over twenty-two periods, and equating and treating by least squares give

Epoch 1859 April 20·5
Period 364·5 days.

Computing from these elements we deduce for times of maxima

1837 May 4·5	Residual error	6 5
1839 May 5·5	„ „	1·5
1854 April 24·0	„ „	25 0
1855 April 23·5	„ „	9·5
1856 April 22·0	„ „	7·0
1859 April 20·5	„ „	18·5

The sum of the squares of the residual errors is 1149.

In 1855 Chacornac found the star to vary 2·7 magnitudes in 34 days, and assuming such rate of variation and mean brightness at maximum 9·5 mag. we may deduce fresh times of maxima as follows:—

1837 May 7.	Hence
1839 April 30.	Epoch
1854 May 19.	1859 March 30·6
1855 April 4.	Period
1856 April 4.	363·4 days.
1859 March 2.	

The residual errors on this supposition are 3·0—6·8—40·4—3·0—0·4—28·6

The sum of the squares of which amounts to 2514.

Hence the first hypothesis is best.

In drawing the conclusions for S from the above premises, the result is rather less satisfactory than for R. It appears most likely, but from our very scanty data we must tread lightly, that the period is liable to fluctuations, as with R, amounting to one-fifteenth of its whole duration—that the star does not attain equal brilliancy at each return, sometimes the 9·5 magnitude, at others not more than the 12th—and that the most probable elements are—

Epoch = 1859 April 21 Period = 364·5 days.

These conditions must, as yet, be deemed merely approximative; yet they promise that, under the able supervision of Chacornac and Pogson, the pair will shortly gain admission into the table of known periodical stars (*see ante*, page 106). We shall assuredly soon know more about the apparent irregularities of the *stellæ versatiles*, and trammel them with fixed laws: but we are yet to ascertain, whether their periodic perturbations are owing to Boulliaud's bodies with opaque regions on their surface—to having been flattened like millstones

(Cycle, I. page 273), with Maupertuis—to their being the effect of total or partial eclipses, as Pigott thought, occasioned by the interposition of some obscure orb—that the variations may be due to Arago's cosmical clouds under revolutionary influence—or whether, with Mr. Norman Pogson, they are downright double-stars, semi-luminous and non-luminous, in orbitual recurrence of the very same phenomena in the same times. *Nous verrons!*

DLXV. σ CORONÆ BOREALIS.

Position	A.B. $145^{\circ}\cdot1$ (w 8)	Distance	$1''\cdot6$ (w 8)	Epoch 1839·67
———	A.C. $88^{\circ}\cdot9$ (w 9)	———	$44''\cdot2$ (w 5)	
———	A.B. $155^{\circ}\cdot9$ (w 9)	———	$1''\cdot8$ (w 5)	——— 1843·35
———	A.B. $162^{\circ}\cdot4$ (w 9)	———	$2''\cdot0$ (w 5)	——— 1846·60
———	A.B. $176^{\circ}\cdot8$ (w 7)	———	$2''\cdot2$ (w 4)	——— 1852·25
———	A.C. $90^{\circ}\cdot0$ (w 7)	———	$46''\cdot3$ (w 6)	

All the conditions mentioned in the Cycle (II. pages 357-8) respecting this interesting binary and triple-star, are now fully confirmed; subject however to the infirmities of instrumental discordances, and the proper motions of a march through space; which last, by the investigation described under η Cassiopæa, was found by the Rev. Robert Main to be—

$$Ɑ\!R = -\ 0''\cdot42 \qquad \text{N.P.D.} = +\ 0''\cdot04$$

Shortly after the Cycle was published, Mr. Hind sent me his recently calculated orbit of σ, by Sir John Herschel's method, in which he increased my period of revolution by 167 years: and these are his general elements—

Perihelion passage	=	1826·48 A.D.
Position of perihelion	=	$88^{\circ}\cdot25$
The node	=	$21^{\circ}\cdot03$
Angle between π and ☊	=	$69^{\circ}\cdot24$
Inclination	=	$25^{\circ}\cdot39$
Ex-centricity	=	0·7256
Semi-axis Major	=	$5''\cdot194$
Mean annual motion	=	$+\ 29''\cdot313$
Period of revolution	=	737 years.

But we are still adrift; for in the XVth volume of the R. A. S. Monthly Notices (pages 90-2), I find a very elaborate orbit by Mr. Eyre B. Powell, of Madras, in which all the epochs of observation are grouped. Though submitted to the graphic method, the results exhibit a startling difference; but, as I give the reference, it is only necessary here to say that the perihelion passage (τ) is 1829·7 A.D., and the period 240 years. Captain Jacob, in the same volume of the Monthly Notices, gives 1831·17 for τ, and 195·12 years for P. This is certainly more yawing than might have been looked for; insomuch that my rough period—however unsatisfactory—may e'en stand a little longer.

DLXXIV. α SCORPII.

Position A.B. 129° 5 (w 1)	Difference Æ = 33ˢ·1 (w 1)	Epoch 1837·35
——— A a 270°·0 (w 1)	Distance —— 3″·5 (w 1)	——— 1857·40

Having heard that Professor O. M. Mitchell, Director of the Cincinnati Observatory, had discovered that Antares was close double, with his splendid refractor of 12 inches aperture, I was rather distrustful of its eligibility for our latitudes; especially as the *comes* was represented to be remarkably minute. But what is found with a large telescope may be seen with a smaller, as Sir William Herschel taught; and very soon after the news arrived from America, Mr. Dawes succeeded in obtaining five nights' observations of its position, and four of its distance; with a mean to this effect—

Position 273°·17 Distance 3″·457 Epoch 1848·02

Owing to accident, or circumstances over which I had no control, I did not obtain a firm glimpse of the stranger till the end of May 1857; and even then the red α was boiling, twitching, collapsing, and dilating among all sorts of variable refractions and the fumes of the neighbouring brickfield. Still the little emerald was seen of about the 8th magnitude; but, as it broke down under very high powers, its place was estimated with a bar-micrometer. Having informed Lord Wrottesley of my success, his lordship attacked the object at its following apparition, with these results, which are a mean of four nights—

Position 275°48′ Distance 3″·296 Epoch 1858·346

In this exploit the telescopes were of $5\frac{9}{10}$, $6\frac{1}{3}$ and $7\frac{3}{4}$ inches in aperture; and the magnifying powers used were between 228 and 450. Now were Antares just as many degrees north of the Ecliptic as he is to the south of it, so as to be clear of his variable and vorticose dancing in the worst part of this latitude's atmosphere, the *comes* would not be a difficult object: but certainly here the opportunities for good measures must be very rare. My son, Charles Piazzi Smyth, in the Report of his "Astronomical Experiment" in 1856, dwells on the astonishing visibility of the companion of Antares from the Peak of Teneriffe (see the Philosophical Transactions for 1858, p. 483). Is the stranger variable?

DLXXVI. η DRACONIS.

POSITION	A.B. 31·0 (*w* 1)	DISTANCE 190″·0 (*w* 1)	EPOCH 1833·62
———	A.a. 145°·0 (*w* 2)	——— 5″·0 (*w* 1)	——— 1857·44

After my measures of the distant companion to η, the Pulkowa telescope picked up a minute *comes* to A, as related in the Cycle (II. page 365). On reconnoitring this object in 1857, though difficult, I found it not extremely so; and, unless the stranger is variable, am rather surprised that it was not caught up before, at Dorpat or elsewhere. Mr. Dawes has kindly handed me these results, obtained by him at Camden Lodge, in Kent—

POSITION 142°42′ DISTANCE 4″·71 EPOCH 1847·41

DLXXVII. λ OPHIUCHI.

POSITION 356°·5 (*w* 5)	DISTANCE 1″·0 (*w* 2)	EPOCH 1839·67
——— 1°·4 (*w* 8)	——— 1″·1 (*w* 3)	——— 1842·50
——— 15°·5 (*w* 6)	——— 1″·2 (*w* 2)	——— 1853·25

This beautiful but close and difficult binary system continues its orbital direct motion, and proves the necessity of adding 180° to Sir William Herschel's angle of 1783, he having quadranted it wrongly (Cycle, II. page 365). I considered, on a graphic projection, that the *annus magnus* might be between 80 and 90 years; but Mr. Hind's more recent computations have

extended that period to 95·88 years. As to the proper motions, in addition to what is advanced at page 366 of the Cycle, I can only add that by Mr. Main's rigid comparison there appeared to be no spacial movement in Æ, but in north polar distance he found it = + 0″·05. The neat blue telescopic star in the south-following quadrant, struck me, more than once, as not being round.

DLXXXIV. ζ HERCULIS.

Position	169°·0 (w 4) Distance 1″·2 (w 1)	Epoch 1838·65	
———	136°·9 (w 6) ——— 1″·2 (w 2)	——— 1842·57	
———	108°·5 (w 3) ——— 1″·0 (w 2)	——— 1848·39	
———	83°·8 (w 4) ——— 1″·3 (w 2)	——— 1852·53	

For the early particulars of this remarkable binary system, see the Cycle (II. pages 369—370); and the singular fact must be added, that the *comes* has been constantly held in view of late years; a circumstance as yet but little to be accounted for. From the measures by Struve, Dawes, and myself, registered in that volume, I made a rough orbit, which discountenanced the rapid race that Struve allotted, yet still gave it a shorter *annus magnus* than that of any other known double star—having an ex-centricity of 0·4186, and a period of about 35 years, as stated. These conditions, considering the nature and degree of the affair, were, I thought, endurable until some more authentic data should accrue.

A few years afterwards a set of orbital elements, computed by M. Yvon Villarceau, on a plan peculiarly his own, appeared in the Astronomische Nachrichten, No. 668, in which the ex-centricity comes out 0·4482 and the period 36·357 years.* Professor Mädler's second orbit yields 0·4545 for ex-centricity, and 31·468 years for period; while Mr. Isaac Fletcher, by a process similar to that which I adopted (see *post*, Story of γ Virginis) obtained 0·4381 for the former element, and 37·21 years for the latter. Now, as the mean of these great years =35·011, I may very well remain satisfied with my result

* Surely the angle for 1840·66, namely 159°·92, must be a misprint in the Nachrichten.

for the present. Some very recent measures have been kindly handed to me by—

Lord Wrottesley,	POSITION	60°·18	DISTANCE	1″·6	EPOCH	1857·46
Isaac Fletcher,	———	59°·24	———	1″·4	———	1857·48

To the proper motions of ζ given in my description, I must now add the values produced by the searching investigations since instituted; they are

Æ = —0″·51	DECLINATION =	+0″·45.—*By Main.*	
—0°·48	———	+0″·41.—*By Mädler.*	

DLXXXVI. η HERCULIS.

POSITION AB	*round* (w 9)	DISTANCE	*round* (w 9)	EPOCH 1835·65		
——— AC	265°·0 (w 2)	———	141″·0 (w 1)			
——— AB	150°·0 (w 1)	———	0″·3 (w 1)	———	1842·53	
——— AB	*round* (w 5)	———	*round* (w 5)	———	1852·47	

This first-class specimen of Struve's *vicinissimæ* was gazed at in 1842 till an impression arose that it was slightly elongated but un-notchable, and therefore under half a second in distance; but the weights assigned show that the idea was vague, and the conclusions worth next to nothing. Nor could I even confirm those conclusions on returning to the charge ten years afterwards; on the contrary, after teazing the object under various powers, and raising spurious discs without any effect, I was obliged to give it up.

DXCI. 19 OPHIUCHI.

POSITION	92°·9 (w 5)	DISTANCE	21″·8 (w 2)	EPOCH	1834·36.
———	92°·2 (w 7)	———	21″·6 (w 4)	———	1857·54.

This pair, though rather wide, forms a difficult object; yet, all the measures being admirably accordant *inter se*, and very satisfactory to the senses at the times of observation, I have great confidence in the results. Indeed, on a consideration of the yaws to which measurements by different persons are liable, the position and distance are all but identical with those of Sir William Herschel in 1783 (Cycle, II. page 374). B. was bright for a 10th, in 1857.

DCII. μ DRACONIS.

POSITION 200°·3 (w 8)	DISTANCE 3″·3 (w 5)	EPOCH 1839·53
——— 191°·6 (w 6)	——— 3″·0 (w 2)	——— 1847·51
——— 190°·7 (w 6)	——— 3″·0 (w 3)	——— 1854·58

Since the registry of this neat object in the Cycle (II. page 380), the retrograde orbital motion is proved, and its claim to binarity established. The distance under all circumstances appears to have slowly diminished in the 73 years since Sir William Herschel measured it; but I have as yet nothing to add to what I already surmised, in respect to its probable *annus magnus*. The proper motions have been again investigated by Mr. Main, entirely from meridional observations made at Greenwich, with these resultant values—

Æ = —0s·011 N.P.D. = —0″·06

DCIV. 36 OPHIUCHI.

POSITION 219°·5 (w 6)	DISTANCE 5″·3 (w 5)	EPOCH 1839·28
——— 216°·6 (w 8)	——— 4″·9 (w 8)	——— 1842·46
——— 213°·8 (w 8)	——— 4″·6 (w 4)	——— 1857·39

Contiguity, brightness, great southern declination, and variable refractions, have combined to render the measures of this star less satisfactory than they ought to have been; but on the whole I think the sentence pronounced (Cycle, II. page 381) of a retrograde change of angle is confirmed, though my last epoch differs so little from the Astronomer Royal's casting vote of 1843. Somewhat disappointed, I wrote to my good friend Lord Wrottesley for its places in the following year, and in due time received those results—

POSITION 213°16′ DISTANCE 4″·446 EPOCH 1858·420

A few years, however, will set all these discordances to rights; but the principal interest of 36 Ophiuchi is its evident connection with 30 Scorpii, and the curious backward march of the pair. This is no hypothesis, but a well established fact, fully verified by official observation and reduction. In the Cycle (II. page 382), I gave the right ascensions and declinations of those two

stars as determined by meridian operations, with good instruments, by the several authorities cited by Bessel, and my own. These it may be eligible to cite again, with the results since obtained by the Astronomer-Royal:—

		$\Delta\alpha$	$\Delta\delta$
FLAMSTEED . .	1690	+ 13′ 32″·4	+ 2′ 56″·0
BRADLEY . . .	1755	. . . .	+ 3′ 02″·7
MAYER	1756	+ 13′ 13″·1	. . . .
PIAZZI	1800	+ 13′ 07″·0	+ 3′ 04″·2
SMYTH	1831	+ 13′ 11″·4	+ 3′ 03″·63
———	1839	+ 13′ 10″·6	+ 3′ 04″·41
AIRY	1858	+ 13′ 09″·0	+ 3′ 08″·47

I deduced the last values from a set of beautiful observations, obligingly made at my request with the new Meridian Circle at Greenwich, of which the following is an exact copy; but in the above statement, I combined both components $\frac{A^1+A^2}{2}$, for the benefit of ready comparison—

Results of the OBSERVATIONS in Æ and N.P.D. of the STARS A¹ OPHIUCHI, A² OPHIUCHI, and 30 SCORPII, made at the ROYAL OBSERVATORY, GREENWICH, in the year 1858.

RIGHT ASCENSIONS.

1858.	A¹ OPHIUCHI.	A² OPHIUCHI.	30 SCORPII.
March 6.	17ʰ 6ᵐ 37ˢ·08	17ʰ 6ᵐ 37ˢ·33	17ʰ 7ᵐ 29ˢ·96
May 18.	37ˢ·06	37ˢ·35	29ˢ·75
,, 19.	37ˢ·10	37ˢ·32	. . . .
,, 25.	37ˢ·13	37ˢ·40	29ˢ·81
June 17.	37ˢ·14	37ˢ·47	29ˢ·81
Means	17ʰ 6ᵐ 37ˢ·10	17ʰ 6ᵐ 37ˢ·37	17ʰ 7ᵐ 29ˢ·83

NORTH POLAR DISTANCES.

1858.			
March 6.	116° 23′ 25″·39	116° 23′ 20″·83	116° 20′ 14″·08
,, 18.	25″·79	19″·83	14″·85
,, 25.	24″·75	19″·89	13″·83
June 17.	24″·57	20″·09	13″·91
Means	116° 23′ 25″·13	116° 23′ 20″·16	116° 20′ 14″·17

G. B. AIRY.
1858, June 26.

Admiral W. H. Smyth.

By some recent elaborate investigations, we are enabled to corroborate the values for proper motion given in the Cycle of Celestial Objects from Piazzi, Baily, and myself, for 36 Ophiuchi, thus:—

In Æ — 0″·55	In Declination — 1″·12.—*Main.*
—— — 0″·54	———— — 1″·133.—*Mädler.*

and for what must be designated the companion star, 30 Scorpii—

In Æ —	In Declination — 1″·15.—*Main.*
—— — 0″·55	———— — 1″·13.—*Mädler.*

It is a very severe trial of meridian observations of contiguous stars in right ascension and north polar distances to convert the differences into angle of position and distance: yet under such treatment the Greenwich results yield—

Position 216°08 Distance 6″·155 Epoch 1858·40

DCV. α HERCULIS.

Position 118°·9 (*w* 9)	Distance 4″·8 (*w* 7)	Epoch 1838·71
——— 118°·7 (*w* 9)	——— 4″·5 (*w* 9)	——— 1842·57
——— 119°·4 (*w* 7)	——— 4″·8 (*w* 5)	1857·63

When the Cycle was published, I was so satisfied of the fixity of this beautiful object—one of the very *pulcherrimæ* of the heavens—that, though I might gaze, I should not have thought of re-measuring it, but for having been aroused by a statement in No. 1103 of the Astronomische Nachrichten. In that number Professor E. Luther publishes the measures of three epochs, made with the far-famed heliometer at Königsberg, according to the method adopted by Bessel, and taken under decidedly favourable atmospheric circumstances; and they run thus:—

Position 123°·53	Distance 4″.99	Epoch 1854·61
——— 123°·82	——— 5″·40	— 1854·67
——— 123°·18	——— 5″·14	——— 1855·31

shewing such a rapid increase in orbit as to call the fullest attention. My last measures, however, restore my confidence in the fixity of this star, though

a highly coloured one; and I can only impute the difference between mine and M. Luther's to some instrumental weakness of a temporary nature, since he followed Bessel's rules as laid down in the first volume of the Astronomischen Untersuchungen. 'Tis a knotty point; but no scandal must be directed against THE Heliometer for stellar measures, however injudiciously it is designated—where stars and not the sun are scrutinized.

The variability of this star, discovered by Sir William Herschel in 1795, has been confirmed; and Argelander's elements of its phenomena will be found in the table of periodical stars (*ante*, page 106). The maximum, minimum, and period do not differ greatly from those in the Cycle (II. page 384).

DCXIII. ρ HERCULIS.

POSITION 308°·9 (w 9)	DISTANCE 3″·7 (w 9)	EPOCH 1839·74
——— 309°·1 (w 7)	——— 3″·8 (w 5)	——— 1847·61
——— 310°·5 (w 7)	——— 3″·5 (w 4)	——— 1853·39

Although I had pronounced upon the fixity of this object (Cycle, II. page 390), various conflicting opinions led me to re-examinations; and the result is, that my opinion is so far shaken, that a slow orbital change may possibly be shown by future measures. Indeed all the recent conclusions agree so closely on the whole, that little doubt can be entertained of a gradual motion in angle. It is a very fine pair, and has been known to be double about 80 years.

DCXXII. 200 P. XVII. HERCULIS.

POSITION 9°·5 (w 8)	DISTANCE 16″·3 (w 6)	EPOCH 1830·71
——— 8°·8 (w 6)	——— 16″.0 (w 5)	——— 1857·65

Having left this neat double star with a doubt upon it, I returned to the charge after a lapse of 27 years, and may now very safely pronounce upon its fixity as an optical object; but still it is liable to movements of another description in space, for no celestial body is absolutely *inerrante*. It is therefore now clear, that the indefatigable Sir William Herschel, in 1783, actually fell into an error of quadrant, as surmised in the Cycle of Celestial Objects (II. page 396).

DCXXIV. μ HERCULIS.

Position 241°·8 (*w.* 9)	Distance 30″·1 (*w.* 5)	Epoch 1837·67
——— 242°·9 (*w.* 7)	——— 30″·8 (*w.* 5)	——— 1857·73

A careful comparison of all the measures of μ Herculis, of which a *resumé* is given in the Cycle (II. page 397), had convinced me so sufficiently of its fixity, that it would not have been brought forward again, but that in 1856 Mr. Alvan Clark, the celebrated American optician, detected B to be close double, with a telescope of his own making, of 7¼ inches aperture. This induced me to take another batch of measures, but all my endeavours to pry into the duplicity of B—itself so small—were *re infecta*. This I do not altogether impute to telescopic inability, although the instrument may really not be competent to deal with it; but I experienced the baleful influence of that Hartwell astronomical nuisance—the Locke Brickfield. At the same time that I was so fruitlessly gazing, my friend Dawes plainly saw it double with his 8-inch object-glass, when the stellar discs were pretty well clued up; it was not excessively close, but about 1″·¾ ±. On the 22nd of June, 1857, he obtained a set of ten measures in position, with powers from 312 to 697—the latter preferred; with which he got also four measures of distance, under slight illumination—"but," he adds, "they were little more than random shots."

The distant telescopic star which follows μ nearly on the parallel, is an excellent object to trim the focus by, for scrutinizing around B; as it is removed from the glare and straggling rays of A.

DCXXX. τ OPHIUCHI.

Position	A B 214°·0 (*w.* 2)	Distance 0″·5 (*w.* 1)	Epoch 1838·58
———	A C 115°·0 (*w.* 3)	——— 82″·7 (*w.* 2)	
———	A B 227°·0 (*w.* 5)	——— 0″·9 (*w.* 1)	——— 1842·52
———	A B 238°·8 (*w.* 5)	——— 1″·1 (*w.* 1)	——— 1855·34

This, one of the closest of double stars in 1783, and *vicinissima* of Struve, is obeying its laws as a binary system, and gradually opening its distance under an increase of angular velocity: insomuch, that, though it is not very steady under definition, it is comparatively easy of measurement of late. Still,

the data hitherto obtained are rather too shaky for the construction of an orbit; but a very rough approximation seems to afford pretty fair evidence for a period of or about 130 years. It demands strict attention.

DCXXXI. 95 HERCULIS.

Position 261°·8 (*w.* 9)	Distance 6″·1 (*w.* 9)	Epoch 1833·78
——— 260°·5 (*w.* 7)	——— 5″·6 (*w.* 6)	——— 1857·63

I was well satisfied of the fixity of this optical pair, on a review of the measures from 1780 (Cycle, II. page 403); but the remarks on the tints of the components, as seen by my son from the Peak of Teneriffe in 1856, again brought it before me, and a new set of positions were taken, which fully confirm that conclusion. A very singular anomaly respecting the colours of this couple will be given in the next chapter of this work; we have therefore here only to subjoin the corresponding places, which were specially taken at my request—

Position 261°·19	Distance 6″·145	Epoch 1857·42.—*I. Fletcher.*
——— 260°·18	——— 6″·093	——— 1857·45.—*Lord Wrottesley.*

DCXXXIII. 70 OPHIUCHI.

Position 126°·5 (*w.* 8)	Distance 6″·25 (*w.* 6)	Epoch 1838·51
——— 122°·4 (*w.* 9)	——— 6″·64 (*w.* 6)	——— 1842·55
——— 119°·7 (*w.* 6)	——— 6″·80 (*w.* 5)	——— 1847·48
——— 114°·9 (*w.* 8)	——— 6″·50 (*w.* 5)	——— 1852·44

A full, true, and particular account of this remarkable binary system appears in the Cycle (II. pages 404 to 409): I have therefore little to add but that the march and influences therein stated are corroborated by each successive set of measures. I stated that round numbers assigned about 80 years for its *annus magnus;* and, though the doctors certainly differ in their elaborate investigations, the results prove that all are shaping a proper course. At present, besides my own estimate, we have—

τ = 1806·88 A D P = 73·76 years.—*Encke.*
τ = 1807·06 A D P = 80·34 years.—*Herschel.*

τ = 1806·82 A D	P = 97·93 years.—*Powell.*
τ = 1808·12 A D	P = 93·10 years.—*Jacob.**
τ = 1806·75 A D	P = 80·61 years.—*Mädler.*
τ = 1807·48 A D	P = 88·48 years.—*Hind.*

There are symptoms of the distance decreasing; but I don't place implicit reliance on my own measures in that respect, though they are entitled to good weight. A diminution of distance with an angular increase, however slight, accords well with the binarity of the object. At present, however, some of the points and even the form of the orbit are little more than conjectural, and we must wait a little longer before we can look upon the results as relatively absolute; but in the mean time here are some good observations, with which I have been favoured since my last epoch was taken—

Position 113°·71	Distance 6″·339	Epoch 1854·73.—*Dawes.*
——— 112°·26	——— 6″·363	——— 1857·42.—*Fletcher.*

By the last German mail which reached our shores, number 1210 of the Astronomische Nachrichten has arrived from Hamburgh; by which I find that another of the barriers supposed to guard sidereal distances, has been successfully assaulted with the Bonn Heliometer. The attack was made on 70 Ophiuchi by Argelander's pupil, assistant, and friend—Dr. A. Krüger—and repeated till the following quantities were wrung from Urania:—

Parallax	+ 0″·169 ± 0″·0103.
Mass of the primary . . .	2″·74 that of our Sun.
Major Axis	29″·34.
Distance	1220000, or 19¼ years of light-time passage.

DCXXXVI. 100 HERCULIS.

Position 2°·8 (*w.* 9)	Distance 14″·1 (*w.* 9)	Epoch 1836·52
——— 1°·6 (*w.* 8)	——— 14″·0 (*w.* 5)	——— 1850·54
——— 2°·5 (*w.* 9)	——— 13″·7 (*w.* 6)	——— 1857·69

From the first two sets of measures which I obtained of this neat object,

* The singular movements of this star have given Captain Jacob no small amount of inquietude; and, after close discussion of all the data, he suspects disturbance from a third invisible companion.

as compared with those of the two Herschels, South, and Struve, (Cycle, II. page 411), as well as by conversions from Piazzi, I was satisfied respecting the optical nature of the components, notwithstanding the discordances made them appear to be backing and filling. Being curious, however, some time afterwards, to see how they turned out for 1850, under scrupulous corrections for precession, secular variation, and proper motion, I took down the British Asssociation Catalogue, converted its right ascensions and polar distances into angles of position and distances from each other, and was somewhat startled at the Frankenstein which I thus produced, namely—

POSITION 119° 46′·5 DISTANCE 7″·45 EPOCH 1850

This was a sad result from such a work, but one which it is necessary to prove wrong; whereupon I not only measured it again as above, but in order to obtain full evidence communicated the alarm to my friends Johnson, Dawes, and Fletcher. And here are their several replies—

POSITION 3°·49′	DISTANCE 14″·2	EPOCH 1856·50.	*Oxford reductions of Æ and N. P. D.*
——— 4°· 15′	——— 13″·956	——— 1857·52.	*Fletcher, wire micrometer.*
——— 2°· 53′	——— 14″·038	——— 1857·58.	*Dawes, wire micrometer.*
——— 1°· 90′	——— 13″·465		*Dawes, double-image.*

There is a minute star, say of the 13th magnitude, in the *sf* quadrant, at an angle of about 120° and a distance of 70″ or 80″, and another of the 11th still further off in the *nf* region, of a greyish tinge. The whole is lively, and forms a neat field under a moderate power.

DCXXXVII. 73 OPHIUCHI.

POSITION 259°·9 (*w* 9) DISTANCE 1″·5 (*w* 5) EPOCH 1838·74
——— 255°·5 (*w* 9) ——— 1″·4 (*w* 6) ——— 1842·39

A *resumé* of my observations since 1834, when I first measured it at Bedford, would yield an inference that a slow retrograde change of angle is taking place with this fine object; but in so close a pair, and so liable to a teasing inflection of light, we must e'en wait awhile. Yet there can be no doubt of

its being now different from what it was when Sir William Herschel described it in 1783, the year of its discovery (Cycle, II. page 411). Since my last epoch was noted, Mr. Fletcher obtained the following places—

Position 256°·17 Distance 1″·5 Epoch 1851·37

DCXLIX. 59 SERPENTIS.

Position	314°·7 (w 6)	Distance	4″·4 (w 6)	Epoch	1835·49
———	314°·2 (w 9)	———	3″·9 (w 9)	———	1842·53

The high weights attached to the last epoch enable me to decide upon the fixity of this neat pair of stars, and I have nothing to add in that respect to my former statement (Cycle, II. page 420) relative to the opticality of the components. As the colours have been questioned, I took an opportunity of re-examining them in a dark field on the 17th of September, 1848, when my record of A yellow and B blue was confirmed. A wide pair of stars follows our 59, nearly on the parallel, and at about 25 seconds' distance.

DCLV. α LYRÆ.

Position	137°·9 (w 8)	Distance	42″·7 (w 4)	Epoch	1837·51
———	140°·3 (w 9)	———	43″·4 (w 9)	———	1843·34

The second epoch of this magnificent insulated star, though very desirable and trustworthy, was taken under one of those provoking annoyances with which the temper is occasionally ruffled. While I was in South Wales, Dr. Lee wrote to inform me that a friend of his had discovered a most minute star between A and B, with the Hartwell telescope. Considering that this object had been long and repeatedly under the gaze and powerful means of the Herschels, the Struves, and other sound authorities, besides not having confidence in the practical ability of the would-be discoverer, I at once expressed my disbelief—adding that it must only have arisen from the flitting of light called a ghost: and such it proved to have been. Officious meddling is, however, a very serious evil to those who are occupied, for even the proving of a negative may occasion a serious loss of valuable time; and Mr. Lassell

was needlessly taxed on this occasion, because he was possessed of a powerful instrument, and is of an obliging disposition. Mr. Maclear will well remember the fruitless hours we expended in 1830, on a flimsy report by a person of this description; who, after having misled us, confessed that what he had taken for a new comet, turned out to be the noted nebula in Andromeda. Sir John Herschel was also victimized in this instance, and I am sorry to say, in consequence of a letter which I wrote to him.

When that intelligent astronomer, Mr. G. P. Bond, gratified me with a visit here in the autumn of 1851, he was enthusiastic in his anticipations of the power of photography; even to prognosticating that it would effect a new era in astronomy. Under this conviction he has sedulously continued his experiments, and achieved the wonderful stellar exploit already recorded in these pages, under ζ Ursæ Majoris (*ante*, page 249). Striding from step to step he arrived at the conclusion, in 1858, that it is possible to distinguish stars by their chemical action: and he cites the curious fact that this star Wega, and Arcturus, though of nearly the same magnitude as seen by a telescope, or to unassisted vision, yet under photographic influence the former surpasses the latter by seven times. It is at once an object of obvious brilliancy and telescopic interest, and a gemmeous *lucida* to a remarkably neat constellation: yet Wollaston allowed it only $\frac{1}{9}$ the light of Sirius!

DCLXI. ε LYRÆ.

Position	AB	21°·9 (*w* 8)	Distance	3″·3 (*w* 5)	Epoch 1839·78
———	CD	152°·8 (*w* 7)	———	2″·5 (*w* 5)	
———	AB	20°·6 (*w* 6)	———	3″·2 (*w* 5)	——— 1842·59
———	CD	150°·9 (*w* 8)	———	2″·6 (*w* 5)	
———	AB	19°·7 (*w* 4)	———	3″·0 (*w* 4)	——— 1853·71
———	CD	148°·1 (*w* 4)	———	2″·5 (*w* 4)	

The same conditions appear still to actuate all the components of this fine quadruple object, as those which I described in the Cycle (II. pages 428-9); and the retrocession of both pairs is abundantly confirmed. Years must pass before the relationship between ε 4 and 5 can be demonstratively established;

but that they are under a common movement I entertain no doubt. It has been said that, by the researches of Argelander, they could not have a common proper motion in right ascension. Now I ask the "gentle reader" to compare those which I have already cited (II. page 429), with these which I now add from the Rev. Mr. Main's investigations (see *ante*, η Cassiopeæ, page 218); and assuredly, on recollecting how such delicate quantities are obtained from meridional operations, he will not be far wrong in pronouncing that there is scarcely any appreciable difference between the ascertained values for the two beautiful systems as yet—

ϵ^1 LYRÆ, 4.		ϵ^2 LYRÆ, 5.		
Æ + 0″·03	Dec. + 0″·07	Æ + 0″·03	Dec. + 0″·08	—*Baily*.
— + 0″·01	—— + 0″·07	— + 0″·22	—— + 0″·09	—*Argelander*.
— — 0″·03	—— + 0″·04	— — 0″·03	—— + 0″·09	—*Main*.
— + 0″·01	—— + 0″·06	— + 0″·02	—— + 0″·08	—*Mädler*.

DCC. β CYGNI.

POSITION	55°·6 (*w* 9)	DISTANCE	34″·4 (*w* 9)	EPOCH	1837·58
———	56°·2 (*w* 8)	———	34″.1 (*w* 5)	———	1854·67

The relative constancy of A and B, both in angle and distance, from the year 1755, as stated in the Cycle (II., page 450), is here still further confirmed; but the proper motions which were formerly assigned to the leader, are fast disappearing under successive meridional observations. Mr. Baily had decreased Piazzi's amount sensibly, and now Mr. Main's investigation gives no change in declination for A, and only 0ˢ·002 in Æ.

My notation of the brightness of these beautiful stars—A 3 and B 7—is taken from Piazzi, whose right ascensions and declinations of them reduce so well on my table in the Cycle. But in 1851 Lieut. Gilliss, of the United States Navy, wrote to me from Chili, that he "was sure" B was of the 5th magnitude: and certainly on the next examination which I made, by my method of appreciation, assuming A as of the third degree of brightness, B might be raised to the 6th. Is it variable? There are several telescopic stars in the field of view with power 240, and one part may be said to be stippled

with star-dust, which, however, in no way interferes with the colours of the pair before us, as recorded in the Cycle, namely, A topaz-yellow and B sapphire-blue. Since then the tints have been pronounced to be

A Golden yellow.	B Greenish blue.	*By Lord Wrottesley.*
A Crocus yellow.	B Greenish blue.	*By Mr. Dawes.*
A Orange yellow.	B Greenish.	*By Mrs. Smyth.*
A Rich yellow.	Brilliant blue.	*By Mr. Fletcher.*
A Pale yellow.	B Blue.	*By Professor C. P. Smyth.*

These differences are more apparent than real, and will mostly disappear before an organized system of observing, and a general chromatic scale. The latest measures handed to me rivet the fixity of the components—though so finely coloured—more strongly; they are as follows:—

POSITION 55°·41′	DISTANCE 34″·370	EPOCH 1857·42.—*Mr. Fletcher.*
——— 55°·26′	——— 34″·557	——— 1857·47.—*Lord Wrottesley.*

DCCXIV. δ CYGNI.

POSITION 30°·9 (*w.* 5)	DISTANCE 1″·5 (*w.* 2)	EPOCH 1837·78
——— 25°·6 (*w.* 8)	——— 1″·8 (*w.* 3)	——— 1842·56
——— 14°·7 (*w.* 5)	——— 1″·5 (*w.* 2)	——— 1852·69

The binarity of this beautiful but delicate and difficult object is now fully established, though the notion of its having undergone occultation and passed its apastron between 1783 and 1826 is abandoned (Cycle, II. pages 456-7). The inflections of light and effects of the large star's glare render the measurement of B by no means an easy task; added to which, some observers consider the *comes* to be variable. Hence the conflicting results which beset the orbit-framer; nothing daunted, however, by contrarieties, Mr. Hind has computed two orbits which satisfy the observations very well, and which nearly agree, except in the node and inclination. The period of revolution appears to be about 180 years, and the excentricity 0·607, so that, though the elements are only approximate, they give a general idea of the orbit's form.

In a letter which I received from Mr. Dawes, dated 17th September, 1857, he says, "Last night was indeed superb. Among other objects I looked at δ Cygni, and saw it well; but, noticing some delicate little stars following

it to the north, I observed something remarkable about one of them, and, after some scrutiny, it turned out to be a close double-star, the smaller of which is about my *minimum visibile*. Pray look at it to-night." This announcement, worded so like one of my own (Cycle, II. page 305), called my attention, and I soon found the new object among the minute followers, or points of light, in the *nf* quadrant; but, after severe gazing and coaxing under various powers, I could not divide nor even wedge it.

DCCXVIII. ζ SAGITTÆ.

POSITION	312°·3 (*w.* 9)	DISTANCE	8″·6 (*w.* 9)	EPOCH	1838·67
———	312°·0 (*w.* 9)	———	8″·4 (*w.* 6)	———	1852·71

The optical relationship of this neat pair must now be considered to be established beyond all reasonable doubt, although the early measures for nearly fifty years indicated the contrary. I was indeed so satisfied with the places of my two first epochs, that I then pronounced a positive decision (Cycle, II. page 459) as to the object's fixity; and, with the further evidence before us, we cannot but infer that the anomalous discordances of angle are owing to errors in the observations themselves. The distances are as accordant as can be expected, with different means used at different times by different observers.

DCCXX. α AQUILÆ.

POSITION 323°·1 (*w.* 8) DISTANCE 152″·6 (*w.* 5) EPOCH 1834·81

My opinions upon this standard star remain as I have expressed them in the Cycle (II. page 261), and years must yet elapse before they can be confirmed or contradicted. But, in the mean time, a minute companion has been found at about 6″ or 7″ following A, for which I have several times searched, but in vain, though minute telescopic stars—and glimpses of light—precede and follow in the northern division of the field, under a power of 240. Charges of variability have been repeatedly made against Altair, but without sufficient evidence to convict; and indeed those who form naked-eye opinions without strict comparison, ought ever to remember the moon's age, the temperature, and the general state of the sky, at the time of gazing.

Æ =	+ 0″.51	Declination =	+ 0″·38.—*Piazzi.*
——	+ 0″·59	——————	+ 0″·31.—*Baily.*
——	+ 0″·56	——————	+ 0″·39.—*Argelander.*
——	+ 0″·54	——————	+ 0″·38.—*Main.*
——	+ 0″·55	——————	+ 0″·37.—*Mädler.*

DCCXXVII. ε DRACONIS.

Position	354°·6 (w. 8)	Distance	3″·1 (w. 5)	Epoch	1833·68
———	356°·3 (w. 6)	———	3″·0 (w. 4)	———	1846·77

Though there seems to be an increase of angle in the two epochs, I am inclined to consider it rather apparent than real, and to retain my opinion of the object's fixity (Cycle, II. page 465). To the value of the proper motions there given by Padre Piazzi and Mr. Baily, the final results of the Rev. Mr. Main's close researches may be added—

Æ = + 0″·23 Declination = − 0″·01

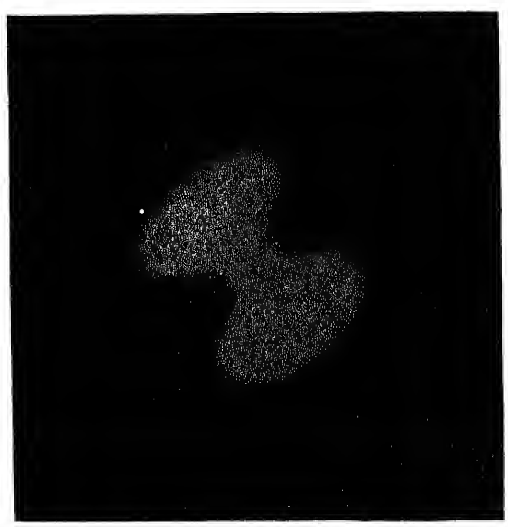
ε Draconis, it will be recollected, was one of the stars selected by Lord Wrottesley for his late investigation of parallax, and that it was omitted in the general summation because it exhibited differences less than its probable errors. This was a great attempt on the part of his Lordship, but science solicits redoubled efforts on the part of amateurs before the question he entered upon can be settled. Perhaps all double stars of short period should be observed throughout the year at or near the equator; at St. Helena, for instance, where some always visible might be selected in order to see how the equation of the angle of position would be affected by the parallax.

DCCXXIX. 27 M. VULPECULÆ.

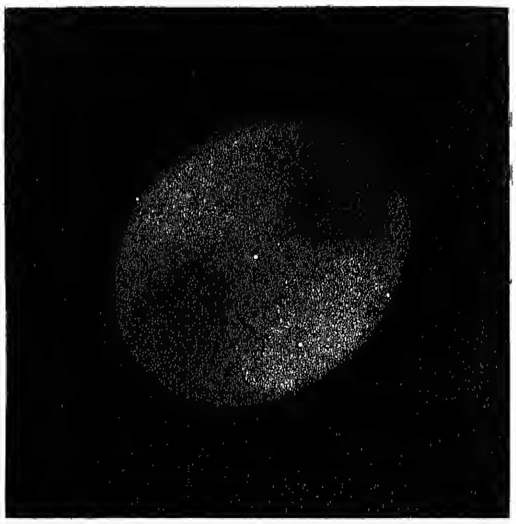
Mean epochs of observation { 1834·62. *At Bedford.*
1857·67. *At Hartwell.*

Since the sketch of this wondrous nebula—which by the way does not well represent the drawing which was handed to the artist—appeared in the Cycle (II. page 466), an astonishing march has been made in space-penetration by telescopes, so as to render us still further eye-witnesses of the

unspeakable glories of the heavens. Some of the nebulæ had resisted all the instruments and magnifying powers which could be brought to bear upon them, so that it could not be decided whether they were optically or physically—apparently or really—nebulous, till the Earl of Rosse led the advance. Though I am still of opinion that there may exist much unformed or luminous matter in space (*see ante*, pages 111-14), I am fully sensible of the powerful effects of that advance on some of the most remarkable bodies of the universe: and of the one before us, his Lordship says—"this is a most difficult object. It requires an extremely fine night, and a tolerably high power; it is then seen to consist of innumerable stars, mixed with nebulosity; and when we turn the eye from the telescope to the Milky Way, the similarity is so striking, that it is impossible not to feel a pretty strong conviction that the nebulosity in both proceeds from the same cause." When Messier first discovered it in 1764, he merely described it as an oval nebula without a star: the difference between which, and as seen in my instrument, is best shown in the following drawing, made under the most favouring circumstances—the definition of the telescope so admirable at the time, that it showed concentric rings with equal distinctness with the eyepiece on either side of the focus. Yet this is the utmost which I could effect—

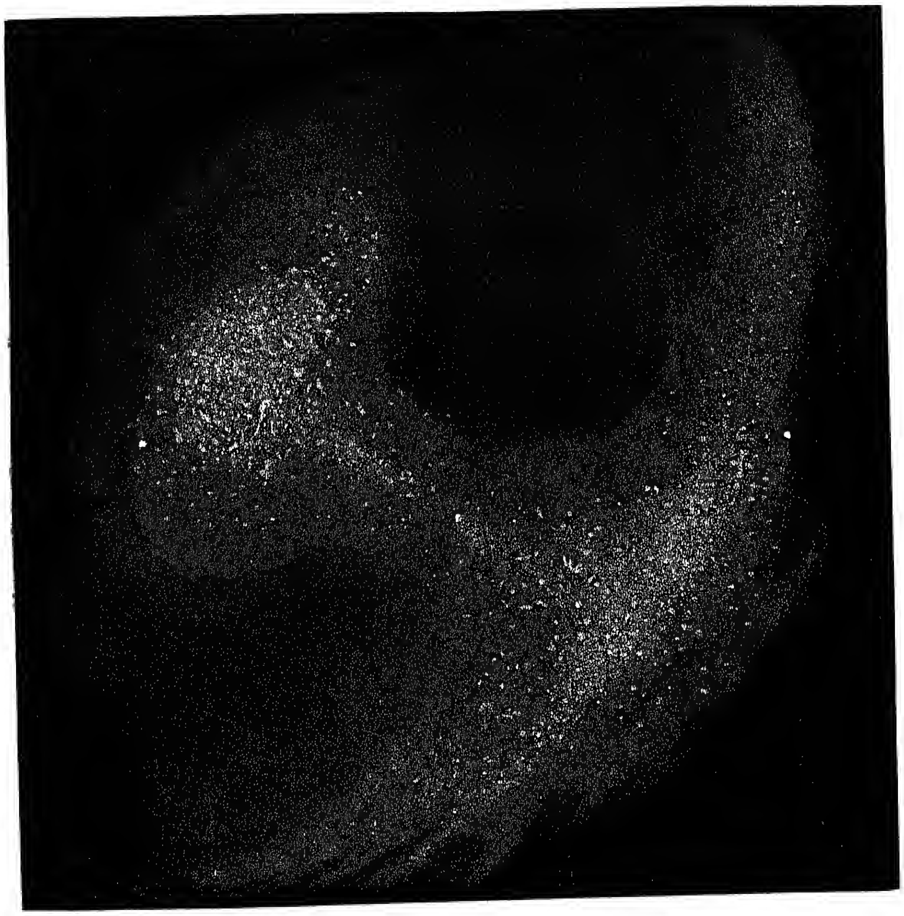


Before my sketch was drawn, the double-headed shot, or dumb-bell nebula, was regarded as a couple of nebulæ with their luminiferous matter running into each other: but Sir John Herschel, with his 20-foot reflector, saw a faint luminosity filling the concavities, and converting the whole into a regular ellipse. Here is an exact copy of his beautiful representation, except that I have altered it to white upon a black ground, and adjusted the axis of symmetry to the vision of my telescope:—



And now we have to complete the progression with Lord Rosse's magnificent ænigma, as to which I repeat that the vastness of its extent is as utterly inconceivable as the dynamical maintenance of its form: and it will be observed, that, while the Parsons-town telescope resolves it into a myriad of suns, the form which presents itself in my drawing is lost. This constitutes a lesson to presumption, which is too prone to leap at conclusions, and catch at causes *per saltum*. When the aged and contemplative Thomas à Kempis breathed his "Raise and refine my mind, pressed down with the dross of earthly desires, and draw my affections up to heaven and heavenly objects,"

he was little aware of the full force which might apply to his ejaculation; for he is not to be envied who can study the following without emotion—



Conversing with Mr. Dawes on this object and its outlying stars, located probably but a short portion of the distance to the nebula, he told me that one I had marked was close double. There being a fine opportunity on the 16th of September, 1857, with a view of testing my telescopic means, I made a dead set at it; yet, under all the coaxing I could exert—with positive,

negative, and single-lens eye-pieces, I could not manage to split it. But I sent a sketch of the field to Mr. Dawes; who replied—"Your diagram is quite correct about the little double-star preceding the dumb-bell nebula; and it forms an interesting example of the fact that two faint objects, though not *very* close together, give the impression of one only. In this case, the distance between the stars may be nearly 3″."

DCCXXXVII. 26 P. XX. ANTINOI.

Position 207°·9 (*w* 9)	Distance 3″·5 (*w* 6)	Epoch 1832·80
——— 210°·2 (*w* 6)	——— 3″·5 (*w* 8)	——— 1847·60
——— 211°·0 (*w* 7)	——— 3″·4 (*w* 5)	——— 1857·71

Instead of a sensible orbital retrogression, as inferred on comparing the observations of 1823 with those of 1783, my middle epoch seemed to imply a direct slow movement. I therefore renewed the attack after an interval of ten years; when the idea of motion received its *coup de grace*, and the fixity of the pair in relation to each other may be considered as satisfactorily established, although apparent anomalies exist in the data. It is to be hoped some one will probe the matter ten years hence, for full confirmation.

DCCLI. 178 P. XX. DELPHINI.

Position AB 256°·1 (*w* 9)	Distance 14″·3 (*w* 7)	Epoch 1835·91
——— AC 125°·0 (*w* 1)	——— 20″·0 (*w* 1)	
——— Bb 210°·5 (*w* 8)	——— 0″·7 (*w* 1)	——— 1842·58
——— AB 255°·5 (*w* 8)	——— 14″·6 (*w* 8)	——— 1857·71
——— Bb 215°·0 (*w* 1)	——— 0″·5 (*w* 1)	

After an interval of fifteen years, this last set of measures was undertaken to find what Mr. Dawes's star b was about; but such are the closeness and nature of the object that I could only get at an angle by preparing a bar across the field of view, and making on that a sort of estimation. In distance, though only guessing each time, it really seemed to be less open at my latest trial than it was in 1842. The manner in which b was detected, and con-

verted the object into a delicate quadruple star, is related in the Cycle (II. page 480). Bb form, in all probability, a binary system.

DCCLIX. 49 CYGNI.

POSITION 48°·8 (w 8)	DISTANCE 3″·2 (w 5)	EPOCH 1838·86
——— 48°·3 (w 6)	——— 2″·7 (w 4)	——— 1850·60
——— 50°·1 (w 8)	——— 2″·5 (w 4)	——— 1857·71

Notwithstanding an angular difference of nearly 10° between Sir William Herschel's results and the later ones, it is doubtful whether there has been any change since 1783: my remarks in the Cycle (II. page 485) must therefore still be applicable. There is a very slight appearance of the components becoming closer; but a diminution of distance, without the increase of angular velocity with which, as a binary, it ought to be accompanied, is against Struve's assumption—"Fortasse in hâc differentiâ motus manifestatur." I cannot but consider the fixity as proven, and that the discrepancies are owing to instrumental want of concord.

DCCLXIII. λ CYGNI.

POSITION AB 104°·3 (w 8)	DISTANCE 84″·9 (w 5)	EPOCH 1834·81
——— Aa 130°·0 (w 1)	——— 0″·7 (w 1)	——— 1843·74

I was inclined to have made another attack upon this delicate and close object Aa, in the winter of 1853, but that, on reconnoitring, it appeared to me under increased difficulty; for to the eye they were not more than half a second of space apart, and moreover some desirable work, within my means, was to be achieved on its follower, 61 Cygni. But I satisfied myself of a rather remarkable fact, in confirming my former opinion, that the components of this ternary group are all blueish, though differing in intensity. Mariotte's mention of blue stars, alluded to in the Cycle (I. page 303), is in his *Traité des Couleurs;* and he thinks their light though faint is pure, without any mixture of exhalations.

DCCLXXI. 429 P. XX. CYGNI.

POSITION 34° 6 (*w* 8)	DISTANCE 2″·1 (*w* 3)	EPOCH 1833·69
——— 34° 9 (*w* 9)	——— 1″·8 (*w* 5)	——— 1857·74

I certainly thought a *quietus* was given to the annual motion of this star; but a passage was pointed out to me in a book of observations published in 1852, stating—"There is some probability in favour of a slow motion, as will be evident from the following series." Upon this I returned to the charge, and, from the evidence again reaped, am prepared to break a lance in favour of its fixity;—keeping in mind, however, the laxity of close measures, the ailments of different instruments, and the infirmities of personal equation.

DCCLXXII. λ EQUULEI. (2 Flamsteed.)

POSITION 225°·6 (*w* 7)	DISTANCE 2″·6 (*w* 4)	EPOCH 1833·72
——— 226°·4 (*w* 8)	——— 2″·5 (*w* 5)	——— 1857·68

This beautiful pair remains apparently unaltered since its discovery by Struve, or rather since his measures of it in 1832. But I was principally led to a re-examination of it by Mr. Dawes's having noted the components "both bluish" in 1846; to my eye, however, they were white in 1833, and, nearly a quarter of a century afterwards, I was obliged to record them pearly white. There are two distant telescopic stars in the *nf* quadrant; and between them and λ I caught up a very minute telescopic point of light, with gleams of star-dust in the field. On sending a diagram to Mr. Dawes for the benefit of his more powerful instrument, he wrote in reply—"On the evening of the 20th (*September* 1857) I obtained a look at λ (2) Equulei. Your *glimpse star* certainly exists, but I am surprised at your even glimpsing it. It comes out, however, positively with high powers; and there is a pretty group of very minute stars nearly preceding your little fellow."

The proper motions of λ—supposed to prove an actual movement, either of the star itself, the solar system in space, or of the two combined—have nearly disappeared under the recent scrutinies.

DCCCLXVII. 61 CYGNI.

POSITION	96°·3 (w 8)	DISTANCE	16″·3 (w 6)	EPOCH	1839·69
———	99°·8 (w 7)	———	16″·4 (w 5)	———	1848·07
———	103°·7 (w 8)	———	17″·0 (w 5)	———	1853·80

One of the most interesting and important revelations of astronomy is undoubtedly 61 Cygni, the motions, the parallax, and the general details of which are to be found in the Cycle (II. pages 494—6): suffice it here to say that the distance appears to be increasing, the path of progression seems still to be nearly in a straight line, and that there are more discordances than might reasonably have been expected in two nearly equal bodies so widely apart, as I have elsewhere remarked. The great amount of proper motion was detected by Piazzi, and has been abundantly confirmed; but it should be attentively watched in future in order to ascertain the plane of motion exactly; and also whether this motion be anywise illustrative of Mosotti's stellar system (Cycle, I. pages 283-4). A *resumé* of all the proper motions at present ascertained, shows the following remarkably accordant values—

	61¹ CYGNI.		61² CYGNI.	
Piazzi . . .	Æ + 5″·38	Dec. + 3″·30	Æ + 5″·30	Dec. + 3″·00
Baily . . .	+ 5″·18	+ 3″·24	+ 5″·28	+ 3″·03
Argelander	+ 5″·11	+ 3″·23	+ 5″·19	+ 3″·02
Taylor . . .	+ 5″·54	+ 2″·93	+ 5″·52	+ 3″·12
Smyth . . .	+ 5″·10	+ 3″·31	+ 5″·21	+ 3″·12
Main . . .	+ 5″·09	+ 3″·22	+ 5″·18	+ 3″·00
Mädler . .	+ 5″·21	+ 3″·22		

results which afford unquestionable proof, as well of the motion as of the crucial excellence of the meridional operations from whence they are derived: and though the illustrious Bessel considered the data, on the whole, to be inadequate to afford a trustworthy set of elements for an orbit, he concluded that the period of revolution must be more than 540 years. This, at present, appears to be a fair relative approximation to truth. Meantime it is a duty with those who have the means, to keep their eye on this most re-

markable object; and I will here close with the mean of some measures handed to me by Mr. Fletcher, since my last batch:—

Position 107° 33′ Distance 17″·766 Epoch 1856·67

The expression which I quoted from Bessel (Cycle, II. page 496) that light occupies 10·3 years to wing away from 61 Cygni's remote *primum mobile*, merits the gravest meditation; since successive transmissions of luminosity plainly indicate the infinitude of space. Thus while light is 10 years in arriving from that little pair of stars, it cannot pass the interval between us and Capella under 70 years, nor from the nebula in Andromeda in 620 times 70 years (*see ante*, page 100)! Hence originated my late friend Baron Humboldt's truly grand conception, that—"much has long ceased to exist before the knowledge of its presence reaches us."

DCCLXXXIX. β CEPHEI.

Position	250°·2 (*w* 9)	Distance	13″·5 (*w* 9)	Epoch	1833·54
———	251°·0 (*w* 9)	———	13″·7 (*w* 9)	———	1843·16

My last observations of this fine object—a standard Greenwich star—sufficiently convinced me of the relative fixity of its components during an interval of 64 years from its discovery by Sir William Herschel. We have been, however, a little at sea as to the colours of this pair; but the time is approaching, it is to be hoped, when a stricter notation will be observed by amateurs in this respect. At present we have as follows—

A White	B White	*Sestini*	1844
A White	B Purplish	*C. P. Smyth* . .	1856
A White	B Blue	*W. H. Smyth* . .	1843
A Yellowish	B Flushed Blue	——— . .	1851

DCCC. 29 AQUARII.

Position	242°·0 (*w* 8)	Distance	4″·5 (*w* 8)	Epoch	1830·78
———	242°·2 (*w* 6)	———	4″·2 (*w* 4)	———	1852·73

This is a very fine object, and I consider its optical condition to be fully

confirmed, although the first measures were made by Struve only about 30 years before my second Epoch. Since my last results Lord Wrottesley, at my request, has obligingly re-examined the pair, to this effect—

Position 242°31′	Distance 4″·065	Epoch 1857·708
——— 242°33′	——— 4″·041	——— 1857.726

Here also, as with β Cephei, we differ as to hues. Captain Jacob, who professes that "his eye is not very acute in distinguishing colours," states that at Poonah, in India, to him they appeared lemon-tinted; and, in order to lead towards greater exactitude, as recommended under β Cephei, I may add that they are thus noted by other observers—

A Red orange	B Same, lighter	*Sestini* . .	1846
A White	B Bright blue	*Lord Wrottesley* . .	1857
A White	B Blue	——— . .	——
A Brilliant white	B White	*W. H. Smyth* . .	1830
A White	B Blueish	——— . .	1852

DCCCXIII. ζ AQUARII.

Position 352°·4 (*w* 7)	Distance 3″·5 (*w* 4)	Epoch 1838·04
——— 348°·9 (*w* 9)	——— 2″·7 (*w* 6)	——— 1842·59
——— 347°·7 (*w* 7)	——— 3″·3 (*w* 4)	——— 1848.02
——— 346°·9 (*w* 8)	——— 3″·2 (*w* 4)	——— 1852·81

From the discovery of this fine binary object in 1779 to the present time, the data obtained have been beset with discordances, which, however, on the whole, are rather apparent than real (Cycle, II. page 518): still these revolving suns are not yet ripe for an accurate orbit,—nor can I say more on that head, at present, than what I have there advanced. On the page above cited, there are discordant values for the proper motions of this curious system; but I am inclined to think Mr. Main's research (see η *Cassiopeæ*) is the best authority. This gives in Æ + 0ˢ·009, and in N.P.D.—0″·03; and the next meridional reductions may prove even this amount to be a vanishing point—especially when certain useless decimal places are knocked off.

DCCCXXI. ξ PEGASI.

Position AB	120°·0 (w 1)	Distance	15″·0 (w 1)	Epoch 1834·79
——— AC	32°·5 (w 2)	———	110″·0 (w 1)	

The Bedford epoch of this most delicate and difficult object is here introduced on account of the discrepancy in rating the components, as mentioned in the Cycle (II. page 522). On a reconnoitre in the autumn of 1857 I find it proper, with my eye and means, still to adhere to the relative brightness and colours I have already registered. At my instance, Mr. Dawes examined ξ on the evening of the 20th of September in that year; and he rated the *comites* B = 11¼, and C the 10½ magnitudes; A being, he thought, rather of a light yellow tint, B blue, and C white. "The vicinity of the large star," he adds, "of course makes B appear of smaller magnitude than it really is: and I think that, upon Herschel's scale, your estimation must be nearly correct—Herschel's 15th being identical with my 10½. It is certainly brighter than his 18th ought to be according to theory, as you observe."

DCCCXXIX. 306 P. XXII. PEGASI.

Position	146°·4 (w 9)	Distance 8″·5 (w 6)	Epoch 1833·88
———	145°·5 (w 8)	——— 8″·2 (w 5)	——— 1849·80
———	147°·0 (w 5)	——— 8″·5 (w 5)	——— 1854·77

Time is still required before any definitive opinion can be pronounced on the optical or physical conditions of this very fine, though delicate, double star. A diminution of 1°·48 in angle between my observations of 1830 and those of 1849, encouraged the notion of a retrograde movement; but it vanished under the results of 1854, though the question still labours under all the incertitude alluded to in the Cycle, (II. page 529). On a fine evening of the autumn of 1857, the colours recorded on the page here cited were confirmed by two pairs of eyes; the whole region, under a darkened field, being rich in telescopic stars, some of them compound, is worthy of being taken into especial care by some amateur who is efficiently armed for the encounter.

DCCCXXXI. π CEPHEI.

POSITION AB	241°·5 (*w* 3)	DIFFERENCE Æ =	11s·8 (*w* 1)	EPOCH	1838·75
——— Aa	330°·0 (*w* 1)	——— ——	1″·8 (*w* 1)	———	1843·77

The manner of finding π Cephei to be a close double-star with a distant companion, is related in the Cycle (II. page 531); but its being caught up was owing to using the macro-micro lens with a dark field of view. In the autumn of 1854, I again scrutinized it, but all imaginable coaxings of eye and instrument could not improve on the estimates of position and distance of the little *comes*, which were made upwards of ten years before: I therefore let well alone. Indeed sharp vision requires that A should be well clewed up, and without flickering rays, rings, or mouldings.

DCCCXXXVI. 69 P. XXIII. AQUARII.

POSITION	272°·1 (*w* 9)	DISTANCE	7″.5 (*w* 7)	EPOCH	1834·79
———	267°·5 (*w* 9)	———	6″·6 (*w* 5)	———	1852·83

The effect of the proper motion of A, as considered probable in the Cycle (II. page 533), has not only not been verified, but has actually produced a variation to the contrary, in its change of place; and, as B has had no special movement assigned to it, ground is afforded for assuming that the pair are physically connected. It is evident that there is a slow retrocession of angle, and perhaps a diminution in the distance; but it will yet require a good interval of time before its phenomena can be pronounced upon with a confidence due to the question. 69 Piazzi is a very neat though delicate pair, and deserves a strict attention: its hues were tested in an unilluminated field, with a positive eye-piece magnifying 240 times, when A was recorded as being of a pale yellow tint, and B lilac, or reddish grey—or plum-coloured.

DCCCXLIX. σ CASSIOPEÆ.

POSITION	323°·7 (*w* 8)	DISTANCE	3″·0 (*w* 4)	EPOCH	1838·96
———	326°·4 (*w* 7)	———	2″·9 (*w* 4)	———	1848·71
———	324°·5 (*w* 8)	———	3″·1 (*w* 2)	———	1858·10

My conviction of the fixity of this elegant pair, with their "colores insignes" in a rich stellar field, was pretty fairly insisted upon in the Cycle (II. page 542); but as an orbital change had been indicated by the observations of Sir William Herschel in 1780 and 1804, to the amount of 11° in the interval, I gave it another shot. The results of this, in common with those of all the recent operations on σ, fully prove the optical character of the object.

Nor can this alteration of angle be aided by the movement which some stars are found to possess, independent of the apparent change of place due to the precession of the equinoxes; for strict comparisons of various meridional observations, at different epochs, assign but small and varying values; and probably the whole amount will disappear before the new Greenwich Circle. At present the result of the latest researches stands thus:

Æ = + 0″·060	DECLINATION = − 0″.02.	—*Baily.*
— = − 0″·015	——— = + 0″.03.	—*Main.*
— = − 0″·007	——— = − 0″.01.	—*Mädler.*

It may here be right to state, that I have just received a copy of Lord Wrottesley's paper to the Royal Society "On the application of the Calculus of Probabilities to the results of measures of the position and distance of Double Stars." I am not at present going to discuss whether the ordinary errors of observation, or the nature and degree of the present manner of observing, demand so elaborate a process; but still the object of the communication is interesting and important, and therefore strongly recommended to the notice of astrometers. My object now is only to notice that σ Cassiopeæ is one of his selected stars; and that in the summary of his results, the position, by the eliminated mean of 112 measures, is = 324°26′ for the epoch 1858·833.

SUCH are the results of the re-examinations of a portion of my Bedford Catalogue at the Hartwell Observatory, wherein I had once plotted to overhaul the whole "Cycle" again, but for certain inconveniences in the going and coming. In the comparison of measures, the reader will have observed that I have not, as heretofore, beat the bush all over Europe for data, because I have

had ready and zealous aid at home, especially in my valued correspondents Dawes, Fletcher, and Lord Wrottesley; in fact, my illustrative estimates have recently been derived from those proportions which were supplied to me expressly for that purpose. Mr. Fletcher, indeed, expressed himself singularly willing to work for the Cycle, to which—as he is pleased to say—he "owes so much;" and in a letter of the 5th of June, 1857, he obligingly observes—"I beg to assure you, that I shall have both pride and pleasure in doing anything that my limited power will admit of, in furtherance of your views." So frank and desirable an offer shall not be cast aside.

It came to pass that, in the autumn of 1858, I repaired with my family on a visit into Cumberland, and, among other places of hospitality, we sojourned at Tarn Bank. With the view of increasing his power as an eye-witness of God's plenipotence and glory, Mr. Fletcher had resolved upon erecting a new observatory, and I was requested to give a casting vote to decide upon its site. This was quickly and satisfactorily arranged, as well as the form and compass of an edifice for the reception of an equatoreal refracting* telescope of $9\frac{1}{2}$ inches in aperture, and $12\frac{1}{4}$ feet in focal length—or distance between the object-glass and the spot where the image is formed by the crossing rays—which had been some time in hand, by the celebrated Mr. T. Cooke of York. This noble instrument, destined to important uses, will be mounted on a principle precisely similar to that which I adopted and described in the Cycle of Celestial Objects (I. pages 336-7), namely, that of a long polar axis, with the telescope on one side of it, and a counterpoise on the other; and, as in the Bedford instance, the moving power is a clock regulated by friction, and driven by a weight which here will have to impel a mass of at least one ton. But an axis of larger dimensions than mine was obviously necessary to sustain an instrument so much greater; and, to ensure smoothness of motion, it was resolved to cast one of a given size, in iron, expressly at the Lowca Engine

* The Dutch painter's noted anachronism of Abraham about to shoot Isaac with a pistol, is not a bit more preposterous than the frontispiece of an astronomical book published in 1858—even in our *enlightened* day—where Galileo is using an equatoreally-mounted telescope, apparently made by Dollond or Troughton.

Works, belonging to Messrs. Fletcher, Jennings, and Co. near Whitehaven. Accordingly, everything being prepared, we all betook ourselves to the foundry to witness the interesting operation. It was a very fine evening on the 27th of October, 1858; but, before describing the process, a preliminary word or two regarding the object and intention may not be considered an unnecessary intrusion, especially as I am persuaded there will be personal zeal and talent, combined with instrumental excellence and strength, brought forward and applied in behalf of scientific philosophy, for that Aristotelian το καλον which sublimity delights in, and brings the conviction that

> If order, greatness, if true beauty lies
> Where perfect most, behold it in the skies!

The polar axis is, as above stated, to be of cast iron, and will consist of *one* casting only; its total length will be 15 feet, with a central cube of 1 foot 5 inches, diminishing to 8 inches in diameter towards the pivots at each end. It will therefore be seen that the central portion is cubical, and the thickness of metal free from the bearings of the declination axis is half an inch; the end portions are conical, and the thickness of metal diminishes from three-eighths of an inch beneath the cube to one-quarter of an inch near the two extremities, where the axis expands to give requisite strength for properly securing the pivots. The declination axis is of hammered iron 4¼ inches in diameter, and case-hardened on the bearings. It bears directly upon the metal of the central cube without the intervention of *bushes* of any kind; consequently it is without any power of adjustment whatever, and it must be presumed that the declination hole is truly at right-angles to the polar axis. The pivots are of hardened steel, and 2½ inches in diameter. At the cube-end there is a cradle firmly welded on; it is four feet in length, and to it the telescope is secured by means of four powerful clamps, or clasps. Thus the cradle and declination-axis form one piece only; the other extremity of the axis carries the counterpoise, so that the declination holes will not wear at opposite sides. To this may be added that the circles are of gun-metal, and of the strongest form, the diameter of each being 3¼ feet, and the graduations on silver. The northern support

of the polar axis is an accurately proportioned Tuscan column of cast-iron, bearing a bracket on its summit with a vertical adjustment; its southern end rests upon a heavy cast-iron frame, which will carry an azimuthal adjustment, and on which the driving clock is to stand. Finally, the whole will be covered by an elliptical dome, having an internal diameter of 18 feet, and revolving on eight railway wheels of 12 inches in diameter.

Such will be the instrument to be directed shortly at the heavens from Tarn Bank, and it is furnished with a double-image micrometer of varying powers, a wire micrometer with positive magnifiers to 1,000, and a battery of negative eye-pieces ranging from 25 to 1,500. Now, as polar axes for such telescopes are not made every day, the reader may like a memorandum of the casting of this one, and a view of the operation shall be appended from a drawing made on the spot by my daughter—Caroline-Mary,* who was in the party assembled at the Lowca foundry, as above stated.

The mould was vertically contained in the square timber case which occupies the centre of the sketch, and reached as far again below the floor-ground, having been judiciously placed near the furnace and its "roaring glow." When the cauldron filled with the molten metal, the right-hand crane raised it till the bottom was even with the top of the mould, and it was then shifted bodily round to approach the left-hand crane, the hook of which was made to seize the cauldron, and by means of the upper chain-tackling was brought

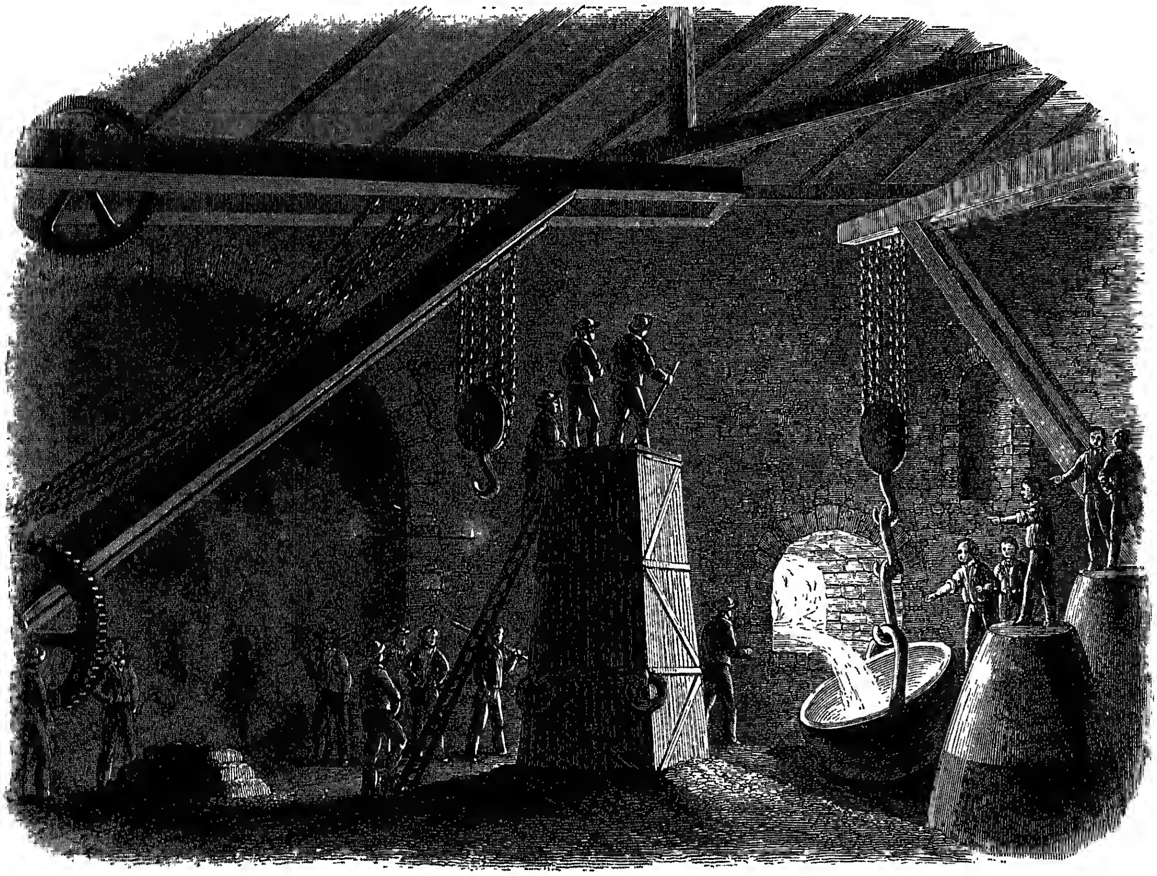
* In Memoriam.

Alas! while this sheet is passing through the press (*September 27th*, 1859,) my beloved, amiable, and accomplished daughter is no more! A most sudden attack of the insidious diphtheria carried her off on Sunday evening (*25th*); within a week of her having been a very type of happy life and cheerfulness, and in less than a month of the day fixed upon for her union with my esteemed and excellent friend and correspondent, Mr. Isaac Fletcher, of Tarn Bank. Dr. Lee was her god-father.

MORS IANVA VITÆ.

to the desired bearing under the able superintendence of Mr. Henry Fletcher. The surface of the bright matter was then well skimmed of every spumous particle, and the ponderous cauldron being accurately tilted, the melted iron was gently poured into the small cavity left for that purpose at the top of the mould.

Notwithstanding the general excitement, not a word was heard till the completion of the cast became evident by the mould rejecting any more metal. It proved to be a faultless casting, and stiff beyond the most sanguine expectation; for, a few days afterwards, the polar axis was suspended by its two pivots horizontally, and laden in the centre to the weight of one ton, with scarcely a hair's breadth of flexure. After amply testing that there was no flaw throughout, the axis was turned, planed, and finished for use. The reader is now prepared for the furnace scene—



CHAPTER VII.

ON THE COLOURS OF DOUBLE STARS.

First the flaming red
Sprung vivid forth; the tawny orange next;
And next delicious yellow; by whose side
Fell the kind beams of all-refreshing green.
Then the pure blue that swells autumnal skies,
Ethereal play'd; and then, of sadder hue,
Emerg'd the deepen'd indigo, as when
The heavy-skirted evening droops with frost.
While the last gleamings of refracted light
Dy'd in the fainting violet away.

THOMSON.

In the Cycle of Celestial Objects (vol. I. page 300-4) I have dwelt upon the colours of double stars; and in the Bedford Catalogue, which forms the second volume of that work, I have assigned such colours to all the objects as struck me at the time of observation. It seems that towards the close of the labours of Signor Benedict Sestini, of Rome, on a very extensive catalogue of stars, my book reached his hands, and led him to form the table which he has appended to his volume,* in hopes that the colours of stars may be more strictly watched. The conclusions which he had already arrived at were, that of two thousand five hundred and forty stars (those of Baily's Catalogue

* A selection from these, twenty-nine in number, was communicated by the late Professor De Vico to Schumacher at Altona, in 1848; who published the details in No. 684 of the Astronomische Nachrichten.

observed at Rome) the yellow stars are about half the total number, and equally distributed; the white stars are one-fifth, in scattered portions; and the orange rather more than one-fifth. The red and the blue are rare from the Pole to 30° of north declination; the blue then become numerous ($= \frac{1}{7}$) to the Equator, especially from Æ 18^h to 20^h; and the red abound from 0 to 30 south declination, and Æ 16^h to 20^h.

On Signor Sestini's instituting the comparisons which he has described in his work, he carefully recorded the dates, as a step to ascertaining whether the colours may or may not be found to vary with time. Political furor—a deadly foe to science—drove De Vico, Sestini, and other distinguished members of the Collegio Romano, from their country; and the two former sought and found an asylum in America. In passing through London, Professor De Vico had given me notice of the use to which my Cycle had been applied; and I afterwards received a copy of Sestini's work, with the following letter from him in English, dated George-town College, March 19th, 1849:—

"Soon after I had completed this collection of observations, I conceived the design of sending a copy to you, not only as a mark of respectful esteem, but also for having profited by your Celestial Cycle in the arrangement and comparison of the observations that I myself had made in the observatory of the Roman College. But the unmerited expulsion and exile that I and my brethren have suffered, have obliged me to leave almost everything, and the printed copies of the little *Memoria* remained in the printer's hands, where I think they are yet. Luckily, a while ago, I found a few copies in a trunk, sent to me after the death of our dear De Vico. Therefore I send you now what I could not send before, and at the same time, and without troubling you, I would desire to know what you advise upon this sort of observations. And, as I have the same objective glass of Cauchoix that I have used in Rome to collect these observations, I desire to know if you approve a review, not entirely but sufficiently general in order to examine if the difference of the climate have any influence on the colour of stars. You can also, if you please, suggest more advisable means than those I have adopted, and I should be very happy if the opportunity of having the same and so good an objective class that I have used for ten years in Italy could give me the means of lending some advantage to the science: what so sincere a lover of this beautiful science as you are can easily find. On another occasion, and when I become better acquainted with the English language, I hope to thank you for all your favours and kindness. Then also I will tell something of the new observatory of this George-town College, erected and very well furnished by the care of M. Curley, a very able and industrious professor, my colleague.

"The observations made and published with my first *Memoria* are reprinted and collected

together with all the others of this second *Memoria*. Nevertheless, if I had here a copy, I would join to the second, and I would send both to you."

In reply to Signor Sestini, I expressed my satisfaction with the course of his inquiries, as they might yet be of great value in a very interesting branch of physical phenomena. He should, however, remember that the colours I had recorded, were frequently noted after the eye was fatigued and biassed by previous working in an illuminated field, and reading minute divisions on graduated micrometer-circles. There were also the imperfections of the eye, the materials of the object-glass, and the various atmospheric media to be considered, before any crucial exactness could be expected; but that still, under every objection, approaches to satisfactory conclusions must inevitably follow a stricter attention to the subject. I therefore recommended occasional references to the heavens for this object only, with the eye kept in sharp order for the purpose, having carefully experimented the capacity of that organ in strictly identifying the many gradations of colour. Many of the tints of stellar companions would of course turn out to be merely complementary contrasts; but the inherent colours would become additionally valuable, as strict observation upon them, under efficient means, advanced. Indeed, I added, it may be considered as fully proved, that the hue of some stars is not the effect of contrast, but a physical reality.*

The unfortunate exiles—De Vico and Sestini—obtained a refuge in Georgetown, on the banks of the Potomac, in the United States; hoping for employment in the newly-erected observatory there. Shortly afterwards, in furtherance of instruments and other scientific objects, the Professor recrossed the Atlantic ocean; but, worn with fatigue and anxiety of mind, he died in London on the 15th of November, 1848. Though usually known as a successful comet-hunter, De Vico was alive to all other branches of celestial research; and he had promised me that, whenever opportunity offered, he would follow up "i colori insigni delle stelle."

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* Secchi *(Osservatorio del Collegio Romano)* says—"Until now a means has been wanting to decide the degree of colour of double stars with precision. If I do not deceive myself I have succeeded, at least to indicate a means by which we can determine it without mistake in centuries to come: this means is derived from the

The question is pregnant with interest; and, as I consider, it may be advantageously encountered by any diligent, sharp-sighted amateur who, possessed of a good telescope and inclined only to easy and pleasing work, is nevertheless zealous of being useful in the cause of knowledge. I have therefore subjoined the list of chromatic comparisons of Sestini's observations with mine; to which I have added those colours which I have since noted at Hartwell. Several of them were confirmed in direct comparison by Dr. Lee; it was an occasion on which the eyes of ladies were prized, and the tints were mostly pronounced without reference to the older records. Here follow the details:—

Stars.	Cycle No.	SMYTH.		SESTINI.		SMYTH.	
		Epoch.	Colours.	Epoch.	Colours.	Epoch.	Colours.
35 Piscium . .	VII.	1837·9	*A. Pale white* *B. Violet*	1844·8	*A. Yellowish* *B. Azure*	1850·7	*A. White* *B. Purplish*
113 P. O. Ceti . .	XVII.	1832·8	*A. Cream yellow* *B. Smalt blue*	1845·8	*A. Yellowish* *B. Azure*	1849·7	*A. Yellowish* *B. Fine blue*
146 P. O. Ceti . .	XXIII.	1837·9	*A. Pale topaz* *B. Violet*	1845·8	*A. Orange yellow* *B. Dull azure*	1849·7	*A. Yellow* *B. Flushed blue*
η Cassiopeæ . .	XXIX.	1843·2	*A. Pale white* *B. Purple*	1845·6	*A. Yellow* *B. Orange*	1850·6	*A. Dull white* *B. Lilac*
65 Piscium . .	XXXI.	1838·2	*A. Pale yellow* *B. Pale yellow*	1844·8	*A. Yellowish* *B. Azure*	1850·8	*A. Pale yellow* *B. Pale yellow*
ψ Piscium . .	XXXVII.	1833·9	*A. Silvery white* *B. Silvery white*	1844·8	*A. Fine azure* *B. Fine azure*	1849·7	*A. Flushed white* *B. Pale white*
α Ursæ Minoris .	XLIV.	1838·2	*A. Topaz yellow* *B. Pale white*	1845·6	*A. Yellowish* *B. Azure*	1849·6	*A. Yellow* *B. Dull white*
ζ Piscium . .	XLVII.	1839·0	*A. Silver white* *B. Pale grey*	1844·8	*A. Yellow* *B. Dingy yellow*	1849·7	*A. White* *B. Greyish*
37 Ceti	XLVIII.	1838·9	*A. White* *B. Light blue*		*A. Yellow* *B. White*	1849·7	*A. Creamy white* *B. Dusky*
ψ Cassiopeæ . .	LII.	1836·3	*A. Orange tint* *B. Blue*		*A. White* *B. White*	1850·2	*A. Golden yellow* *B. Ash-coloured*
85 P. I. Piscium .	LIV.	1837·0	*A. Yellow* *B. Pale blue*	1844·8	*A. Yellow* *B. Azure*	1855·4	*A. Pale yellow* *B. Bluish*
γ Arietis . . .	LXXII.	1837·9	*A. Bright white* *B. Pale grey*	1844·9	*A. White* *B. White*	1850·7	*A. Full white* *B. Faint blue*

spectrum of the electric spark (*questo mezzo è desunto dallo spettro della scintilla elettrica*), and I will shew it in conclusion. My attempts hitherto, for want of time and apparatus, have not been reduced to practice."

Stars.	Cycle No.	Smyth.		Sestini.		Smyth.	
		Epoch.	Colours.	Epoch.	Colours.	Epoch.	Colours.
λ Arietis . . .	LXXVI.	1830·9	*A. Yellowish white* *B. Blue*	1844·9	*A. White* *B. Pale azure*	1857·7	*A. Pale yellow* *B. Flushed blue*
α Piscium . .	LXXXI.	1838·9	*A. Pale green* *B. Blue*	1844·8	*A. White* *B. White*	1850·8	*A. Greenish* *B. Pale blue*
γ Andromedæ .	LXXXII.	1843·3	*A. Orange* *B. Emerald green*	1846·5	*A. Red orange* *B. Lighter red*	1850·3	*A. Deep yellow* *B. Sea-green*
14 Arietis . . .	LXXXVI.	1833·9	*A. White* *B. Blue*	1844·9	*A. Yellowish* *B. Bluish white*	1857·7	*A. Pale white* *B. Grey*
72 P. II. Cassiopeæ	XCVII.	1834·8	*A. Pale yellow* *B. Lilac*	1845·6	*A. White* *B. White*	1857·7	*A. Yellowish white* *B. Purplish*
θ Persei . . .	CIX.	1838·6	*A. Yellow* *B. Violet*	1845·6	*A. Yellow white* *B. Azure*	1840·6	*A. Yellow* *B. Dusky blue*
η Persei . . .	CXV.	1838·8	*A. Orange* *B. Smalt Blue*	1845·7	*A. Golden orange* *B. Azure*	1850·7	*A. Reddish yellow* *B. Blue*
32 Eridani . .	CXLVII.	1843·2	*A. Topaz yellow* *B. Sea-green*	1845·9	*A. Yellow* *B. White*	1850·3	*A. Bright yellow* *B. Flushed blue*
φ Tauri . . .	CLVIII.	1832·8	*A. Light red* *B. Cerulean blue*	1845·8	*A. Golden orange* *B. Azure*	1852·5	*A. Pale red* *B. Blue*
χ Tauri . . .	CLX.	1831·9	*A. White* *B. Pale sky-blue*	1845·8	*A. White* *B. Azure*	1850·7	*A. White* *B. Grey*
62 Tauri . . .	CLXI.	1835·9	*A. Silver white* *B Purple*	1845·8	*A. White* *B. White*	1850·7	*A. White* *B. Pale purple*
88 Tauri . . .	CLXIX.	1832·9	*A. Bluish white* *B. Cerulean blue*	1845·8	*A. White* *B. White*	1852·5	*A. Bluish white* *B. Blue*
τ Tauri . . .	CLXXI.	1831·9	*A. Bluish white* *B. Lilac*	1845·8	*A. Very white* *B. Azure*	1852·5	*A. Pale white* *B. Violet*
ω Aurigæ . .	CLXXIV.	1833·8	*A. Pale red* *B. Light blue*	1845·7	*A. White* *B. White*	1850·7	*A. Flushed white* *B. Light blue*
62 Eridani . . .	CLXXV.	1831·9	*A. White* *B. Lilac*	1845·9	*A. Light yellow* *B. Azure*	1852·5	*A. Pale white* *B. Flushed blue*
14 Aurigæ . . .	CLXXXVIII.	1832·8	*A. Pale yellow* *B. Orange*	1845·7	*A. White* *B. Azure*	1850·7	*A. Greenish yellow* *B. Bluish yellow*
23 Orionis . . .	CXCVII.	1835·2	*A. White* *B. Pale grey*	1845·9	*A. Yellowish* *B. Bluish white*	1850·2	*A. Creamy white* *B. Light blue*
111 Tauri . . .	CXCVIII.	1832·9	*A. White* *B. Lilac*	1845·9	*A. Yellowish* *B. White*	1857·7	*A. Pale white* *B. Lilac*
118 Tauri . . .	CCV.	1838·9	*A. White* *B. Pale blue*	1845·9	*A. White* *B. White*	1850·2	*A. White* *B. Bluish*
ϑ Orionis . . .	CCXI.	1836·1	*A. Brilliant white* *B. Pale violet*	1845·9	*A. Yellowish* *B. Very white*	1850·2	*A. Pale white* *B. Flushed white*
λ Orionis . . .	CCXV.	1843·1	*A. Pale white* *B. Violet*	1845·9	*A. Yellowish* *B. Bluish white*	1850·2	*A. Pale yellow* *B. Purplish*
ι Orionis . . .	CCXVIII.	1832·1	*A. White* *B. Pale blue*	1845·9	*A. Slightly yellow* *B. Azure*	1852·5	*A. Pale white* *B. Bluish*

Stars.	Cycle No.	SMYTH.		SESTINI.		SMYTH.	
		Epoch.	Colours.	Epoch.	Colours.	Epoch.	Colours.
26 Aurigæ . .	CCXX.	1833·1	*A. Pale white* *B. Violet*	1845·7	*A. Yellowish* *B. Blue*	1849·7	*A. Dusky white* *B. Pale blue*
σ Orionis . . .	CCXXII.	1832·2	*A. Bright white* *B. Bluish*	1845·9	*A. Yellow* *B. Azure*	1850·3	*A. White* *B. Grey*
ζ Orionis . . .	CCXXIII.	1839·2	*A. Topaz yellow* *B. Light purple*	1845·9	*A. Yellowish* *B. Azure*	1850·3	*A. Yellow* *B. Flushed blue*
γ Leporis . . .	CCXXV.	1832·1	*A. Light yellow* *B. Pale green*	1845·9	*A. Orange yellow* *B. Orange red*	1852·2	*A. Pale yellow* *B. Flushed*
8 Monocerotis .	CCXLV.	1834·2	*A. Golden yellow* *B. Lilac*	1845·9	*A. Pale yellow* *B. Yellowish*	1850·8	*A. Yellow* *B. Flushed blue*
15 Geminorum .	CCXLVII.	1832·0	*A. Flushed white* *B. Bluish*	1845·9	*A. Orange* *B. Yellowish*	1852·4	*A. Pale white* *B. Ash coloured*
20 Geminorum .	CCLII.	1834·0	*A. Topaz yellow* *B. Cerulean blue*	1845·9	*A. Yellowish orange* *B. Yellow*	1849·7	*A. Yellow* *B. Pale blue*
π² Canis Majoris .	CCLXX.	1834·1	*A. Flushed white* *B. Ruddy*	1845·9	*A. Yellowish* *B. Reddish*	1851·3	*A. Bluish white* *B. Ruddy*
α Geminorum .	CCXCII.	1843·1	*A. Bright white* *B. Pale white*	1845·9	*A. Yellowish* *B. Yellow*	1849·2	*A. Very white* *B. Pale white*
ζ Cancri . . .	CCCXV.	1843·1	*A. Yellow* *B. Orange tinge*	1846·0	*A. Yellow* *B. White*	1849·2	*A. Yellow* *B. Bright yellow*
φ² Cancri . . .	CCCXX.	1843·2	*A. Silvery white* *B. Silvery white*	1846·0	*A. Yellowish* *B. White*	1849 2	*A. White* *B. Pale white*
υ' Cancri . . .	CCCXXI.	1843·2	*A. Pale white* *B. Greyish*	1846·0	*A. White* *B. White*	1849·2	*A. White* *B. Dusky white*
72 P. VIII. Argo Navis	CCCXXIII.	1830·8	*A. Red* *B. Green*	1846·1	*A. Orange red* *B. Yellow*	1851·8	*A. Orange* *B. Bluish green*
108 P. VIII. Hydræ	CCCXXVI.	1839·1	*A. Pale yellow* *B. Rose tint*	1846·0	*A. Orange* *B. Orange*	1849·2	*A. Full yellow* *B. Flushed*
ι Cancri . . .	CCCXXXVI.	1836·2	*A. Pale orange* *B. Clear blue*	1846·0	*A. Fine orange* *B. Azure*	1851 3	*A. Dusky orange* *B. Sapphire blue*
τ' Hydræ . . .	CCCLX.	1831·9	*A. Flushed white* *B. Lilac*	1846·1	*A. Yellow* *B. Yellow*	1851·3	*A. Pale white* *B. Dusky*
6 Leonis . . .	CCCLXIII.	1832·2	*A. Pale rose tint* *B. Purple*	1846·0	*A. Fine orange* *B. White*	1851·3	*A. Flushed yellow* *B. Pale purple*
7 Leonis . . .	CCCLXIV.	1832·2	*A. Flushed white* *B. Violet tint*	1846·0	*A. Rather yellow* *B. White*	1851·3	*A. Bluish white* *B. Pale violet*
9 Sextantis . .	CCCLXXI.	1832·2	*A. Blue* *B. Blue*	1846·0	*A. Dingy orange* *B. Dingy orange*	1851·3	*A. Flushed blue* *B. Pale blue*
35 Sextantis . .	CCCLXXXIV.	1839·1	*A. Topaz yellow* *B. Smalt blue*	1846·1	*A. Pale yellow* *B. Pale yellow*	1849 2	*A. Rich yellow* *B. Cerulean blue*
54 Leonis . . .	CCCXCI.	1839·3	*A. White* *B. Grey*	1846·0	*A. Yellow* *B. White*	1851·3	*A. Silvery white* *B. Ash-coloured*
φ Leonis . . .	CCCCV.	1831·2	*A. Pale yellow* *B. Violet*	1846·2	*A. Pale yellow* *B. White*	1851·3	*A. Pale yellow* *B. Dusky red*

Stars.	Cycle No.	SMYTH.		SESTINI.		SMYTH.	
		Epoch.	Colours.	Epoch.	Colours.	Epoch.	Colours.
90 Leonis . . .	CCCCXXI.	1835·4	*A. Silvery white* *B. Purplish*	1846·1	*A. White* *B. White*	1851·3	*A. Silver white* *B. Pale purple*
δ Corvi . . .	CCCCXLVI.	1831·3	*A. Pale yellow* *B. Purple*	1846·3	*A. Slightly yellow* *B. White*	1851·3	*A. Light yellow* *B. Purple*
24 Comæ Berenicis	CCCCLI.	1836·4	*A. Orange colour* *B. Emerald tint*	1844·4	*A. Gold* *B. Azure*	1851·3	*A. Orange* *B. Lilac*
143 P. XII. Virginis	CCCCLIII.	1833·3	*A. Pale yellow* *B. Greenish*	1846·3	*A. Red* *B. Azure*	1851·3	*A. Yellowish* *B. Flushed blue*
12 Canum Venaticorum	CCCCLXVI.	1837·4	*A. Flushed white* *B. Pale lilac*	1844.5	*A. Yellow* *B. Blue*	1850·5	*A. Full white* *B. Very pale*
ζ Ursæ Majoris .	CCCCLXXX.	1839·3	*A. Brilliant white* *B. Pale emerald*	1844·5	*A. White* *B. Yellowish*	1849·2	*A. White* *B. Pale green*
ι Bootis . . .	DVIII.	1838·2	*A. Pale yellow* *C. Creamy*	1844·5	*A. Orange yellow* *C. Azure*	1850·6	*A. Light yellow* *C. Dusky white*
π Bootis . . .	DXVII.	1836·5	*A. White* *B. White*	1844·4	*A. Yellow* *B. Less yellow*	1850·6	*A. White* *B. Creamy*
10 Hydræ . . .	DXIX.	1831·5	*A. Pale orange* *B. Violet tint*	1846·4	*A. Yellow* *B. Yellow*	1851·4	*A. Deep yellow* *B. Reddish violet*
212 P. XIV. Libræ	DXXIV.	1833·4	*A. Straw colour* *B. Yellow*	1846·3	*A. Orange* *B. Orange*	1851·4	*A. Yellow* *B. Dusky*
44 Bootis . . .	DXXIX.	1842·5	*A. Pale white* *B. Lucid grey*	1844·5	*A. Orange* *B. Orange*	1858·6	*A. Pale yellow* *B. Dusky*
δ Bootis . . .	DXXXVII.	1835·5	*A. Pale yellow* *B. Light blue*	1844·5	*A. Gold yellow* *B. Yellowish azure*	1851·3	*A. Yellow* *B. Lilac*
μ^1 Bootis . . .	DXLII.	1832·3	*A. Flushed white* *B. Greenish white*	1844·5	*A. Yellow* *B. Yellowish azure*	1850·6	*A. Yellowish* *B. Greenish white*
ζ Coronæ Borealis	DXLIX.	1842·6	*A. Bluish white* *B. Smalt blue*	1844·5	*A. White* *B. White*	1850·6	*A. Flushed white* *B. Bluish green*
51 Libræ . . .	DLVIII.	1842·5	*A. Bright white* *B. Pale yellow*	1846·4	*A. Orange* *B. Orange*	1850·6	*A. Creamy white* *B. Pale yellow*
β Scorpii . . .	DLIX.	1835·4	*A. Pale white* *B. Lilac tinge*	1846·4	*A. Yellow* *B. Whitish*	1851·4	*A. Yellowish white* *B. Pale lilac*
κ' Herculis . .	DLX.	1835·4	*A. Light yellow* *B. Pale garnet*	1844·5	*A. Yellow* *B. Orange*	1851·3	*A. Pale yellow* *B. Reddish yellow*
ν Scorpii . . .	DLXI	1831·5	*A. Bright white* *B. Pale lilac*	1846·5	*A. Yellowish* *B. White*	1850·6	*A. Pale yellow* *B. Dusky*
σ Scorpii . . .	DLXVIII.	1833·3	*A. Creamy white* *B. Lilac tint*	1846·5	*A. Yellow* *B. White*	1851·4	*A. Dusky white* *B. Plum colour*
236 P. XVI. Scorpii	DXCIII.	1833·4	*A. Yellowish white* *B. Pale green*	1846·5	*A. Yellow* *B. White*	1851·4	*A. Creamy white* *B. Greenish*
μ Draconis . .	DCII.	1839·5	*A. White* *B. White*	1844·5	*A. Yellow* *B. Azure*	1850·7	*A. White* *B. Pale white*
36 Ophiuchi . .	DCIV.	1842·4	*A. Ruddy* *B Pale yellow*	1846·5	*A. Orange yellow* *B. Orange yellow*	1851·4	*A. Ruddy tint* *B. Yellowish*

Stars.	Cycle No.	SMYTH.		SESTINI.		SMYTH.	
		Epoch.	Colours.	Epoch.	Colours.	Epoch.	Colours.
39 Ophiuchi . .	DCVII.	1838·5	*A. Pale orange* *B. Blue*	1846·5	*A. Orange* *B. Yellow*	1851·4	*A. Pale orange* *B. Bluish*
ν Serpentis . .	DCX.	1832·6	*A. Pale sea-green* *B. Lilac*	1846·5	*A. Yellow* *B. Red*	1851·4	*A. Silvery tint* *B. Native copper*
ρ Herculis . .	DCXIII.	1839·7	*A. Bluish white* *B. Pale emerald*	1844·4	*A. Yellow* *B. Deeper yellow*	1850·5	*A. Greyish* *B. Greenish*
53 Ophiuchi . .	DCXVIII.	1836·5	*A. Bluish* *B. Bluish*	1844·5	*A. White* *B. Azure*	1850·5	*A. Greyish* *B. Pale blue.*
95 Herculis . .	DCXXXI.	1833·8	*A. Greenish* *B. Cherry red*	1844·5	*A. Gold yellow* *B. Gold yellow*	1851·3	*A. Pale green* *B. Reddish*
70 Ophiuchi . .	DCXXXIII.	1842·5	*A. Pale topaz* *B. Violet*	1845·9	*A. Gold yellow* *B. Gold yellow*	1849·5	*A. Topaz yellow.* *B. Purplish*
ε Draconis . .	DCLXXII.	1837·9	*A. Orange yellow* *B. Lilac*	1844·5	*A. Fine orange* *B Copper colour*	1851·3	*A. Orange* *B. Lilac*
15 Aquilæ . . .	DCLXXVIII.	1831·6	*A. White* *B. Lilac tint*	1846·5	*A. Reddish* *B. Red orange*	1851·4	*A. Yellowish white* *B. Red lilac*
28 Aquilæ . . .	DCXC.	1831·4	*A. Pale white* *B. Deep blue*	1844·5	*A. White* *B Yellow*	1851·4	*A. Dusky white* *B Lilac blue*
β Cygni . . .	DCC.	1837·6	*A. Topaz yellow* *B. Sapphire blue*	1844·5	*A. Orange gold* *B. Azure*	1849·6	*A. Golden yellow* *B. Smalt blue*
ε Sagittæ . . .	DCCIV.	1833·8	*A. Pale white* *B Light blue*	1844·5	*A. Yellow* *B. Bluish yellow*	1850·6	*A. Faint yellow* *B. Bluish*
54 Sagittarii . .	DCCV.	1837·6	*A. Yellow* *B. Violet*	1846·5	*A. Orange* *B. White*	1850·7	*A. Yellow* *B. Pale lilac*
ζ Sagittæ . . .	DCCXVIII.	1838·6	*A. Silvery white* *B. Blue*	1844·5	*A. Yellowish white* *B. Azure*	1850·6	*A. Flushed white* *B. Cerulean blue*
56 Aquilæ . . .	DCCXXII.	1834·6	*A. Deep yellow* *B. Pale blue*	1846·5	*A. Yellow* *B. Yellow*	1850·6	*A. Yellow* *B. Bluish*
κ Cephei . . .	DCCXLIII.	1838·8	*A. Bright white* *B. Smalt blue*	1844·6	*A. Yellowish* *B. Azure*	1851·3	*A. Pale yellow* *B. Blue.*
γ Delphini . .	DCCLXII.	1839·7	*A. Yellow* *B. Light emerald*	1844·5	*A. Orange* *B. Yellow*	1850·7	*A. Golden yellow* *B. Flushed grey*
ε Equulei . . .	DCCLXX.	1838·8	*A. White* *B. Lilac*	1844·5	*A. Gold orange* *B. Azure*	1851·4	*A. Pale yellow* *B. Bluish lilac*
1 Pegasi . . .	DCCLXXXII.	1833 9	*A. Pale orange* *B. Purplish*	1844·5	*A. Orange* *B. Azure*	1851·4	*A. Deep yellow* *B. Lilac blue*
β Cephei . . .	DCCLXXXIX.	1843·1	*A. White* *B. Blue*	1844·6	*A. White* *B. White*	1851·3	*A. Yellowish* *B. Flushed blue*
3 Pegasi . . .	DCCXC.	1837·8	*A. White* *B. Pale blue*	1844·5	*A. White* *B. Yellow*	1850·5	*A. Flushed white* *B. Greyish*
ε Pegasi . . .	DCCXCIV.	1833·6	*A. Yellow* *B. Blue*	1844·5	*A. Gold yellow* *B. Azure*	1851·4	*A. Bright yellow* *B Blue lilac*
μ Cygni . . .	DCCXCV.	1839·6	*A. White* *B. Blue*	1844·5	*A. Yellow* *B. More yellow*	1850·6	*A. White* *B. Pale blue*

Stars.	Cycle No.	SMYTH.		SESTINI.		SMYTH.	
		Epoch.	Colours.	Epoch.	Colours.	Epoch.	Colours.
29 Aquarii . . .	DCCC.	1830·8	*A. Brilliant white* *B. White.*	1846·5	*A. Red orange* *B. Same, lighter*	1852·7	*A. White* *B. Bluish*
ξ Cephei . . .	DCCCII.	1839·6	*A. Bluish* *B. Bluish*	1844·6	*A. White* *B Yellowish*	1851·1	*A. Flushed* *B. Pale lilac*
ζ Aquarii . . .	DCCCXIII.	1842·6	*A. Very white* *B. White*	1845·8	*A. Orange yellow* *B. Pale yellow*	1849·2	*A. Flushed white* *B. Creamy*
δ Cephei . . .	DCCCXV.	1837·7	*A. Orange tint* *B. Fine blue*	1844·6	*A. Orange* *B. Azure.*	1849·2	*A. Deep yellow* *B. Cerulean blue*
τ^1 Aquarii . . .	DCCCXXII.	1838·7	*A. White* *B. Pale garnet*	1845·8	*A. White* *B. Azure*	1849·2	*A. Pale white* *B. Flushed*
ψ^1 Aquarii. . .	DCCCXXXIII.	1834·9	*A. Orange tint* *B. Sky blue*	1845·8	*A. Gold* *B. Azure*	1850·8	*A. Topaz yellow* *B Cerulean blue*
94 Aquarii . . .	DCCCXXXIV.	1838·9	*A. Pale rose tint* *B. Light emerald*	1845·8	*A. Orange yellow* *B. Orange*	1850·8	*A. Orange tint* *B. Flushed blue*
101 P. XXIII. Cassiopeæ	DCCCXXXIX.	1830·9	*A. Light yellow* *B. White*	1844·6	*A. White* *B. Yellowish*	1852·7	*A Pale white* *B Yellowish*
107 Aquarii . . .	DCCCXLIV.	1832·8	*A. Bright white* *B. Blue*	1845·8	*A. Yellowish white* *B. Yellowish*	1850·7	*A. White* *B. Purplish*

All the differences in the above list are subject to several doubts, and many of the records have been noted without a very strict attention to the question. I have, in the Cycle, mentioned the many disagreements between the tints of stars as given by Sir William Herschel and myself; and the anomaly is partly accounted for by his peculiarity of vision, and partly by the tone of metal in his reflectors. But I am at a loss why refractors should differ so widely as here shown; and therefore hope the subject will be more closely pursued than it has hitherto been. I am aware that the notations independently made at various epochs will vary in term, though to the observer's eye they may mean nearly the same tint; but some of the differences mentioned by Signor Sestini in his interesting Memoir are singularly striking. He says—"Now, beginning with the companion of γ Andromedæ, we have Smyth emerald-green and Sestini white; but Herschel and Struve at another date call it azure. Moreover, observing it again after a lapse of two years, and four years after Smyth, I

find it no longer white, but a strong blue!" And again—"Now see B (95) Herculis; according to Smyth one is greenish and the other red; but we think them both a golden yellow. A Ophiuchi, by Smyth, one ruddy and the other pale yellow; but we take them to be both orange. The contrary occurs in ι Bootis, the components of which by Smyth are both pale yellow; but we deem one to be orange and the other azure."

Under the circumstances to which I have already alluded, I am not at this stage disposed to theorise on the objects thus brought into juxta-position: and the colours of double-stars must be much more accurately assigned, and more ably experimented upon, before we can admit that the nature and character of those suns can possibly change in short periods. Sir David Brewster observes, that there can be no doubt that in the spectrum of every coloured star certain rays are wanting which exist in the solar spectrum; but we have no reason to believe that these defective rays are absorbed by any atmosphere through which they pass. And in recording the only observation perhaps yet made to analyse the light of the coloured stars, he says—"In the orange-coloured star of the double-star ζ Herculis, I have observed that there are several defective bands. By applying a fine rock-salt prism, with the largest possible refracting angle, to this orange-star, as seen in Sir James South's great achromatic refractor, its spectrum had the annexed appearance (in the Campden Hill Journal), clearly shewing that there was one defective band in the red space, and two or more in the blue space. Hence the colour of the star was orange, because there was a greater defect of blue than of red rays." This instance shows, that an approximation by instrumental means to the spectra of the brighter stars ought not to be despaired of.

In the year 1856, on my son's going to the island of Teneriffe to make his "Astronomical Experiment," it occurred to me that it would be a singularly fine opportunity to test sidereal polychromy; since it would be marked from a spot where some thousands of feet of the grossest portion of our atmosphere are eliminated. He accordingly scrutinized the following stars from the "Cycle" for me: those on the 29th of July and 4th of August were examined with the

5-foot Sheepshanks' equatoreal, at Guajara, a height of 8870 feet; and those of September 4, 5, and 6, were made with the Pattinson telescope of 7·25 inches aperture, and parallactic movement, at the Alta Vista, where the altitude is 11,000 feet. These are the registered results:

July 29th	α Herculis	A. *Cadmium yellow*	B. *Greenish*
	39 Ophiuchi	A. *Pale yellow*	B. *Faint blue*
	5 Serpentis	A. *Pale yellow*	B. *Warm lilac*
	ρ Herculis	A. *White*	B. *Bluish*
	95 Herculis	A. *White*	B. *White*
	70 Ophiuchi	A. *Pale yellow*	B. *Greenish*
	α Lyræ	A. *White*	B. *Violet*
August 4th	α Herculis	A. *Cadmium Yellow*	B. *Greenish*
	95 Herculis	A. *and* B. *both yellow with tinge of bluish-green*	
	70 Ophiuchi	A. *Yellow*	B. *Warm green*
	5 Aquilæ	A. *Pale yellow*	B. *Bluish.* C. *Blue*
	28 Aquilæ	A. *White*	B. *Blue*
	β Cygni	A. *Pale yellow*	B. *Blue*
	186 P. Antinoi	A. *Yellow*	B. *Blue*
Sept. 4th	α Scorpii	A. *Coppery red*	B. *Blue*
	α Herculis	A. *Orange*	B. *Greenish*
	ζ Sagittæ	A. *Yellow*	B. *Blue*
	α^2 Capricorni	A. *Yellow*	B. *Blue*
	1 Pegasi	A. *Yellow*	B. *Blue*
	β Cephei	A. *White*	B. *Purple*
	3 Pegasi	A. *Whitish*	B. *Warm grey*
	ζ Piscium	A. *Yellow*	B. *Grey*
	γ Arietis	A. *White*	B. *White*
	λ Arietis	A. *Pale yellow*	B. *Light lilac*
	α Piscium	A. *White*	B. *White*
Sept. 5th	α Aquilæ	A. *Pale yellow*	B. *Grey*
	γ Delphini	A. *Cadmium yellow*	B. *Greyish tinge*
	τ′ Aquarii	A. *Light yellow*	B. *Pale violet*
	α Piscis Australis	A. *White*	B. *Blue*
	ψ′ Aquarii	A. *Cadmium yellow*	B *Blue*
	94 Aquarii	A. *Yellow*	B. *Light warm lilac*
	101 Cassiopeæ	A. *Light yellow*	B. *Grey,* b. *blue*
	———	C. *Blue*	D. *Violet*
	107 Aquarii	A. *Pale yellow*	B. *White*
	35 Piscium	A. *Yellow*	B. *Pale violet*
	113 Ceti	A. *Rich yellow*	B. *Warm grey*
	γ Arietis	A. *Light yellow*	B. *Light yellow*
	222 Arietis	A. *Grey*	B. *Blue*
	———	C. *Lilac*	D. *Yellow*
	α Piscium	A. *White*	B. *White*
	γ Andromedæ	A. *Orange*	B. *and* C. *Green*
	32 Eridani	A. *Orange*	B. *Greenish*

	σ Cassiopeæ	A. *Pale yellow*	B. *Light blue*
	35 Piscium	A. *Pale yellow*	B. *Pale lilac*
	113 Ceti	A. *Yellow*	B. *Warm grey*
Sept. 6th	146 Ceti	A. *Yellow*	B. *Pale violet*
	η Cassiopeæ	A. *Yellow*	B. *Indian red*
	65 Piscium	A. *White*	B. *White*
	ψ' Piscium	A. *White*	B. *White*
	ζ Piscium	A. *White*	B. *Reddish*

In the above list, there seems to be a very general similarity of eye-judgment between my son and myself; whence it would appear that the difference made by 11,000 feet of lower atmosphere on the colours is not so great as might have been anticipated. But the most striking and inexplicable difference is that of 95 Herculis; for, in the observations at the Peak of Teneriffe, the tints of the two stars—though not quite the same at each examination—were judged to be common to both, and the impression was ratified by the evidence of some Spanish visitors at the astronomical aerie. Not a little taken aback, however, by the unexpected announcement—the more unexpected in consequence of the general agreement which existed throughout the list, even in some of the most delicate hues—I took the earliest opportunity of returning to the charge, when there I found A apple green and B cherry red, as recorded by me nearly a quarter of a century before! To avoid all suspicion of bias I invited my colleagues to the task, and soon received an answer from Mr. Dawes, saying—"On referring to my colour-estimations, I find that they agree very nearly with your own." Lord Wrottesley pronounced A to be greenish and B reddish in 1857·46; and at the same epoch Mr. Fletcher reported that A, to his eye, was light green and B pink; while to his brother H. A. Fletcher one was bluish green and the other orange, and to Mr. T. W. Carr A was either light blue or green, and B dull red. To add to the perplexity of the instance, Sestini saw them both golden yellow at Rome, in 1844; while his colleague, De Vico, in the memoirs of the "Osservatorio del Collegio Romano," in the same place and with the same instrument, dubs them "rossa e verde." Assuredly this is most passing strange, since the means in these cases were pretty equally powerful, and *chromatic personal equation*—or the faculty in

a greater or less degree of appreciating differences of colour, cannot be entitled to consideration with the discrepancies of 95 Herculis.

Under our present practice, various difficulties are presented, for the designation of hues uttered in parlance by several persons often means the same tints in different words, and these will not always quadrate with the chromatic language of photologists. The wonder, however, is not so great that, without a due nomenclature, we should differ so much from each other, and even from ourselves at different dates, but that, with such an unorganized practice, so many instances should coincide relatively. In many cases the difference of colour in the components of a double star are real; but when they are merely complementary, the fainter of the two may possibly be a white star which appears to have the colour complementary to that of its more brilliant companion. This is in consequence of a well-understood law of vision, by which the retina of the eye being excited by light of a particular colour, is rendered insensible to less intense light of the same hue,—so that the complement of the whole light of the fainter star finds the retina more sensible than that part which is identical in colour with the brighter star; and the impression of the complementary tint accordingly prevails. But the accurate perception of the colour of a celestial body often depends as much on the condition of the eye when the object is seen, as upon the object itself; and possibly the achromatism of the object-glass, which, being adapted to the solar spectrum may not be suitable to the spectrum of a star, ought to be taken into account; as well as a nice adjustment of the eye-piece, to lead to a discrimination between real and illusive appearances. The powers of colours in contrasting with each other, agree with their correlative powers of light and shade; and such are to be distinguished from their powers individually on the eye, which are those of light alone. It may assist the memory of the inexperienced observer, to remind him that the primary colours and their complementaries are in these relations—

Red . . .	*Green*
Blue . . .	*Orange*
Yellow . .	*Violet*

and from these a scale may be readily drawn up of the subsidiary tints and their opposites (the male and female lights of Milton*), through all the twistings of Iris: and if he will bear in mind the laws of harmonious alliance and contrast of colour - that yellow is of all hues the nearest related to light, and its complementary violet or purple to darkness—that red is the most exciting and positive of all colours, and green the most grateful—that blue is the coldest of all hues, whilst orange is the warmest—much of the apparent mystery of harmonizing the multiplied tints of primary, secondary, and tertiary colours, will be readily accounted for.

In the present incertitude, it is suggested that variations in colour may be owing to variations in stellar velocity; but in this case would there not also be as palpable a variation in brightness? If it shall be found that the tints actually vary, the comparative magnitude should also be carefully noted, to establish whether a variability in brightness accompanies the changes of colour. Sestini, however, does not view the matter in this light: he holds that the undulations of each colour arrive in succession to our eyes, and that therefore at last, when they have all reached us, they will appear white. In arguing the circumstances necessary for the case—as the strength of vibrations with their number and velocity in a given time—he cites Huyghens, Euler, Young, Fresnel, and Arago. Quoting Herschel's data, he observes, that five hundred and thirty-six billions of vibrations cause us to see yellow, while six hundred and twenty-five billions exceed the number that shews blue: that is, when the tangential celerity of the moving star in relation to its companion comes at its maximum to equal one-thirteenth of that of light. Its green colour will change insensibly into yellow on increasing its distance, and then,

* The notion of male light being imparted by the Sun, and female light by the Moon, is as old as the hills. Pliny, in his CYCLOPÆDIA (*lib. ii. cap.* 100 *and* 101,) mentions it as a condition "which we have been taught;" and he details the influences of the masculine and feminine stars. Here, probably, Milton imbibed the hint to which I alluded in the Cycle (I. page 301)—

"Other suns, perhaps,
With their attendant moons, thou wilt descry,
Communicating *male* and *female* light."

receding through the same steps, it will again become green; beyond which, as it approaches the eye, it will become a full blue; finally, in the inverse order, it will turn to green, and so on. But this explanation is not admissible, as may be readily shown: for instance, if we accelerate the velocity of the star to one-fifth of that of light, we shall have the number of vibrations corresponding to red = four hundred and eighty-one billions, and seven hundred and twenty-one billions, which exceeds that of violet. In this supposition, the green star when furthest from its companion will become red, and when approaching it must be of an intensely strong violet tinge; after which, owing to its circular orbit, it will in receding again become green, thus passing through all the colours of the spectrum. These are the ratios—

$$536 : 625 :: 1-\tfrac{1}{13} : 1+\tfrac{1}{13} :: 12 : 14 :: 6 : 7.$$
$$481 : 721 :: 1-\tfrac{1}{5} : 1+\tfrac{1}{5} :: 4 : 6 :: 2 : 3.$$

Admitting these and the like grounds, as the laws of new stars and binary systems may be somewhat elucidated thereby, I strongly recommend repeated examinations of the brightness and colours of stars to the well-equipped amateur, who is also happily possessed of a good eye, perseverance, and accurate notation. But even thus prepared, I would advise him, before entering upon the undertaking, to study well the third chapter of the great work of my highly-esteemed friend Sir John Herschel, on the Uranography of the Southern Hemisphere: it treats of Astrometry, or the numerical expression of the apparent magnitudes of the stars. In a more advanced state of this question the measurement of brightness should always accompany that of colours, since a change in the one might possibly produce variation in the other: and who can say that numerical measures may not be made with such extreme precision hereafter, that the distance of stars thereby may be given? The observer must not however be unnerved by the difficulties, some of them apparently insuperable, which beset the inquiry: nor by the philosopher's assertion that "nothing short of a separate and independent estimation of the total amount of the red, the yellow, and the blue rays in the spectrum of each star would

suffice for the resolution of the problem of astrometry in the strictness of its numerical acceptation; and this the actual state of optical science leaves us destitute of the means even of attempting with the slightest prospect of success." This is indeed a damper to our argument, so far at least as stars differing in colour are concerned; but perseverance in a good cause has often been rewarded with marvellous accomplishments,—and it is well to remember that

> By many blows that work is done,
> Which cannot be achieved with one.

These remarks will hardly be impinged upon in practice by taking one objection to the facts upon which Sestini's theory is founded, namely, the velocity of the stars; since, in the present day—even admitting proper motions and translations in space to their fullest extent—it is not necessary to consider the possible rate of sidereal movements as capable of bearing any sensible ratio to the speed of light. In citing the case of the orbital velocity of the companion of a double star, he should have applied it to α Centauri, an object of which we know all the elements, its distance from us and from each other in miles, the mass of the components as compared to our Sun, their quantity of light as compared to the same, and the periodic time;—all these we know to a greater degree of confidence than those of any other similar body. Now the theory fails upon this test; for the mean orbital velocity of the companion may be assumed as 2·5 miles per second, while Sestini's limits of $\frac{1}{13}$ and $\frac{1}{5}$ of the velocity of light would make it fifteen thousand and thirty-eight thousand miles in the same time The velocity of light assumed here is, however, it must always be remembered, that of the Sun; that determined by direct observations of the solar orb itself, or by the eclipses of Jupiter's satellites, whose reflections still give us solar light, and traversing the same medium, whatever it be, filling the planetary spaces. But we may reasonably expect, and, indeed, the experiments detailed above, on the spectra of different stars, appear to indicate, if not actually to prove, that the light of some of the stars is absolutely of a distinct nature, and radically of a different composition, to

that of the Sun; while the media which the rays have to pass through may be of a kind unknown in any part of the whole extent of our planetary circles, and of a nature of which we are at present profoundly ignorant.

Evidently, therefore, when the speed of transmission of the stellar rays comes into play, we may have to deal with velocities very different to that on which our correction for aberration—which depends upon solar light—is founded: the speed of transmission of which element, the velocity of electric light, and the speed of sidereal light, appear to depend upon, or be affected by, different causes. Granting that, however, and to the widest extent, extending even the somewhat doubtful experiments which have been made on the velocity of electric light as compared with the solar, and on the transmission of ordinary light through air and through the denser medium of water; still there is nothing as yet to show, that we are likely to meet with any kind of light moving at so slow a rate, as to bear the proportion which Sestini's theory requires to the actual speed at which any star has been found to move.

There is, however, another way in which the peculiar habitude of rays of light may produce a difference of colour in a star, and make it even run through the whole of the colours of the spectrum from one end to the other and back again, in a greater or less space of time according to the particular circumstances of the case. This will occur if the different coloured rays of which the white beam is composed, have intrinsically in themselves, or by reason of the nature of the medium which they traverse, any difference in the velocity of their transmission.

According to the Newtonian doctrine of "emission," the separate colours are actually produced by different degrees of velocity: and he concluded—from experiment—that the transparent parts of bodies, according to their several sizes, reflect rays of one colour and transmit those of another. But, according to the "undulating" theory, which has since been shown by Young and Fresnel to be far more probable than the other, if not really to be the true theory, the various tints are produced by means of undulations of different lengths; and the physicists have even been able to measure the

comparative extent of these minute waves, or undulations, and have assigned them as follows:

	Parts of an Inch.
Red	0·00002582 *
Orange	0·00002319
Yellow	0·00002270
Green	0·00002073
Blue	0·00001912
Indigo	0·00001692
Violet	0·00001572

Now, though this by itself may say nothing with respect to the rapidity with which each undulation may be transmitted, it renders the probability of such a difference extremely great; and, though that difference be so very small that there is no hope of ever being able to make it manifest in any scientific apparatus of even the most delicate description, yet, on account of the great distance of the stars, the effect may become at length very sensible. For although the difference of the rate of propagation of the waves of each ray may be the smallest conceivable quantity, yet, if that different rate be kept up during the whole one thousand years that we suspect must be occupied by the light of some of the stars in reaching us, notwithstanding that it may travel on the average one hundred and ninety-two thousand miles in a second, it is manifest that, after continuing to grow during so great a length of time, a very decided effect may at last may be produced.

If a new star suddenly appears in some part of the sky, the rays of light immediately travel off to announce the fact everywhere, and to us amongst the number of other orbs; and it matters not whether the light consist in the emission of particles, or the propagation of waves of different orders, as many of

* These lengths of an undulation lead to the astounding decision, that, on viewing a red object, the membrane of the eye trembles at the rate of 480,000000,000000 of times in every two beats of a seconds' pendulum! The researches and discoveries of Huyghens, Young, Malus, Fresnel, Arago, Poisson, Airy, Wheatstone, and others, have rendered the hypothesis of an undulatory propagation of light almost a demonstrated truth. "It is a theory," says Herschel, "which, if not founded in nature, is certainly one of the happiest fictions that the genius of man has yet invented to group together natural phenomena, as well as the most fortunate in the support it has received from all classes of new phenomena, which at their discovery seemed in irreconcileable opposition to it. It is, in fact, in all its applications and details, one succession of *felicities*; inasmuch that we may almost be induced to say, if it be not true, it deserves to be."

Arago's "couriers" as there are different colours in the spectrum are sent off with the intelligence; and, if one is able to accomplish the great intervening distance between the star and us in a less space of time than the others, and so arrive before them, we shall see the star of that colour first, say blue. In that case the next to arrive would be the yellow, and then arriving and mixing with the blue, already come, would make the star change from blue to green; while the red, arriving last of all, and joining themselves to the existing green, would at length make the star appear white; and, if it preserved the same lustre, it would ever after continue white.

But be it recollected that, in the ideas evoked by the discrepancies of colour-estimates, I am only throwing out suggestions, not advocating an hypothesis; and it must be admitted that variations of colour ought to accompany variations of brightness, but such variation of hue has not hitherto been detected in stars that notably vary in splendour. With this confession, we will proceed in the vision thus conjured up, and return to the celestial body in white; only reminding the reader, that little is correctly understood of light in its causes and principles of existence, and that Bacon has told us—*rectè scire est per causas scire.*

If the above-cited star is shown for only an instant of time as an electric spark, then we should see it varying through each of the different colours, blue, yellow, and red, separately and distinctly. Allowing that, for example, the blue ray was to traverse the space between the star and ourselves in three years, the yellow in three years and one week, and the red in three years and two weeks, and supposing the above to apply only to the central portion of each coloured ray, which should gradually vary with filaments of different velocities so as to join insensibly with those of the neighbouring one; then, three years after the striking of this stellar spark, we should see a blue star appear in the sky, and last for one week, then the star would appear yellow during another week, and red during another; after which it would be lost altogether. If there be actual separations between the different colours, as is more than hinted at by the discovery of the black bands in the spectrum,

then the star, after appearing of one colour, might disappear for a time before the next colour began to arrive.

Again, if a star which has existed for ages be on a sudden extinguished, the rays last emitted will be the couriers to announce the fact; and, supposing the star to have been white, three years afterwards (in the above particular example), the last of the blue rays having arrived before the last of the others, the blue will be deficient in the star, and from white it will become orange; after a week all the yellow ones will have come in, and the star will be red; and, when the final rays of this colour have arrived, it will totally disappear.

But if the star shines permanently, and has so shone from time immemorial, then, whatever might be the difference of time elapsing between the blue and red rays shot out from the star at the same instant reaching us, we should see the star white; for blue and yellow and red rays of different dates of emission would all be reaching our eyes together.

This case can be exemplified by looking through a prism at a white surface of unlimited extent and equal brightness, when it will be seen as white as before; for the multitudinous spectra formed by all the component points of the whole surface overlaying each other, the red of one coming to the blue and yellow of others, will form white light as completely as if the three colours of one point be concentrated together again. Here was Goethe's error: he gazed at a white wall through a prism, and, finding it white still, kicked at Newton's theory to produce an absurd one of his own. But had he looked at the edges of the wall—which is a similar case to the birth or death of a star—he would have seen the blue half of the spectrum on one side, and the red on another: everything, in fact, with a sensible breadth, will have coloured borders, blue on one side and red on the other. If one part of the wall, however, be brighter than another, the strong blue of that thrown on to the fainter red of another, will give that a bluish tinge, and *vice versa:* and so with the stars; if their brightness should alter, or, in the common but singularly erroneous parlance, their magnitudes vary, the strong blue of a bright epoch arriving with faint red of a dull period, will make blue appear to us as the

predominating colour; will cause indeed the star's light to appear decidedly blue at one time, and, *mutatis mutandis*, red at another, although all the while the star's colour may not really have altered at all; but may have been really, and would have appeared to observers close by, as white as ever, varying only in quantity and not in quality. Real alterations in colour may doubtless occur, but evidently may often be only consequences of alterations in brightness, which may be brought about by many regular and periodical phenomena, and certainly do not require the introduction of any such startling reason as the conflagration which was lugged in to explain the tints through which the variable star of 1572 passed, as it gradually died out of the sky, where it had so suddenly appeared a few months previously. Of this, at least, we may be certain, that there are periodical variations in the brightness of the stars, and that some alteration of colour should thereby be produced; but whether to a sensible extent or not, is only to be determined by experiment. β Persei has been selected by Arago as a favourable instance for testing this matter by observation, because it changes so very rapidly in brightness in a short space of time; but, though he did not succeed in detecting any alteration of colour, we must not despair; for, while on the one hand his means of determining the colour seem to have had no sensible degree of exactness, it is easily possible to assume such a difference of velocities for the various coloured rays of the star, and such a distance for them to traverse, as should completely annihilate the expected good effect of the quickness and frequent recurrence of the changes in this particular star. Many other stars might indeed be picked out where the natural circumstances are more promising, while the perfection of the means of observation would allow of many more still being made subservient to the inquiry.

The failures made here may therefore be regarded in the same light as those in the olden inquiry of finding the parallax of the fixed stars, viz. not as reasons for leaving off, but for trying again more energetically, more extensively, and with more accurate means than before; and, although I may not be prepared just at present to describe any perfectly satisfactory method

of observation, still, as some amateurs desirous of pursuing the subject may like to see such hints as my experience has necessarily given rise to, presented in some rather more practical form, I have thrown them together as follows:

In any method of determining colours of stars, three possible sources of error have to be met: 1. The state of the atmosphere generally at the time in altering the colour of all the stars above the horizon; 2. The effect of altitude in varying on different stars the apparent colour produced by the atmosphere; and 3. The effect on the eye of the necessary quantity of some sort or other of artificial light, for the purpose of writing down or examining the dimensions of the instrument, the face of the clock, &c. &c.

The first can only be eliminated by extensive observation of a number of stars, especially circumpolar ones, all through the year. Although the colours of some stars may vary in a small number of months, weeks, or even days, the mean of them all may be considered to be safely depended on for a tolerably constant quantity; and each star should be examined and tested for its colour every night, by comparison with the mean of all the rest; and where any decided variation appears to be going on through the year, that star should at once be excluded from the standard list, and its difference from the mean of the others stated as its colour for each night's observation.

The second source of error is to be met by observations of the same star through a large part of its path from rising to culminating, or of a number of stars of known colour at various altitudes, combined with a correction something similar to that for refraction, as varying in a proportion not far from the tangent of the zenith distance, and which would consequently require the altitude of every body observed to be noted, as a necessary element in reducing the observations.

Low stars, however, should be eschewed, and each observer should confine himself as far as possible to his zenith stars; for, in addition to the low ones being so much fainter to him, than to one to whom they are vertical, and in addition to the colouring and absorbing effect of the atmosphere increasing so excessively, low down on the horizon, the envelope acts so strongly

there as a prism that, combined with the bad definition prevailing, I have sometimes seen a large star of a white colour really appear like a blue and red handkerchief fluttering in the wind: the blue and red about as intense and decided as they could well be. This shows the extreme importance of noting not only the altitude of the star, which determines also the degree of prismatic effect, but of distinguishing in the observation any difference between the upper and lower parts of the star. In the Sun and Moon, bodies of very sensible breadth, this effect is not so evident; the surface will still be white or coloured uniformly by the atmosphere, and the upper and lower borders will alone show the prismatic colours, half on one edge and the other half on the other, as in the case of the white wall mentioned above; but the star, being merely a point of light, is wholly acted on, and exhibits as complete a spectrum as could be contrived without any of the white or self-compensating intermediate portion.

Combined with this is the colouring effect of the object-glass, and any deficiency of its achromaticity; but these being nearly the same on all the stars, will not affect the difference observed: yet the latter quality of the eye-piece will be of more consequence, unless the star be brought very rigorously into the centre of the field of view, and kept there the whole time that it is under observation. An achromatized eye-piece should be specially used, and its magnifying power always recorded.

The third difficulty may be best counteracted by using one eye for the field of the telescope, and the other for writing down, &c.; having the artificial light used for these purposes as faint, and making them as white as possible, with various other little practical details which will best occur to each observer.

We then come to the grand difficulty: viz. the manner in which the colour is to be determined; the methods are two: first, by the senses; second, by instrumental means. The first is that which has been employed hitherto, and will doubtless still be the only method employed for a considerable time by amateurs; and, though so very vague, yet may—by the education and

the practice of the senses, combined with the corrections above considered—be carried to considerable perfection: but the education must be much more systematic, and the practice much more constant, than they have hitherto been. Nor will the pursuit be unfruitful, even if it only relieves science by proving a negative; but to the zealous there is a hopeful guerdon, because much of the theory of the universe may be finally revealed by this elegant and delicate element.

Some certain standard of colours must be kept and constantly referred to: the colours of precious stones have been used for this purpose; but, though very proper in one point of view, as being by their brightness more comparable to stars than ordinary pigments are, yet astronomers in general have not much acquaintance with anything so valuable and costly; and, if they had, would find that the colour of each star is not certainly to be defined by its name, *i. e.* that under the same name many different colours may be found; and different observers will therefore be giving the same name to stars not resembling each other; in addition to which there is not a sufficient range of colours amongst the precious stones to meet all the cases which occur in nature in the heavens, and they neither admit of being mixed, to form varieties of colour, nor of being modified, to show gradations in their own colour; a most important defect. These qualities, however, are possessed by the water colours of the present day; the greater part of them are very permanent, and the others, which are not so, are capable of being prepared fresh and fresh; the number of colours moreover is great, the combinations that may be formed of them almost endless; and gradations of each may be made, from nearly white to next to black. Not only must a scale of them be had in possession, and frequently referred to, but it must be made and remade by the observer, as a mode of impressing the colours on his memory; and, unless he can carry them in his mind, he need not attempt the chromatic observation of stars; for, as he cannot see the star and his scale of colour at the same instant, and side by side, the estimate of the star depends entirely on the memory.*

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* Chromatography is not so near perfection as the eye and state of art would lead us to suppose it to be; but it

We tried an experiment on chromatic personal equation, in its simplest form, at Hartwell, on a fine evening of the second of July, 1829. Having prepared a stone pedestal in front of the south portico of the house, on which was placed a Gregorian telescope of 5½ inches aperture, a party of visitors, consisting of six ladies and five gentlemen, were invited to gaze upon the double-star Cor Caroli; and they were each to tell me—*sotto voce* to prevent bias—what they deemed the respective colours of the components to be. The first who stepped out by request was my good friend the late Rev. Mr. Pawsey—more addicted to heraldry than to astronomy—who, after a very momentary snatch, flatly declared that he "could make out nothing particular:" but the other spectators were a little more attentive to the plan proposed, and their respective impressions were thus noted down in the large Hartwell Album:—

Mrs. Tyndale	A. *Pale white.* B. *Violet tint.*	Miss Mary Anne	A. *Paleish yellow.* B. *Blue.*
Mrs. Rush	A. *Yellowish cast.* B. *Deadish purple.*	Mr. Rose	A. *Cream colour.* B. *Violet cream.*
Miss Honor	A. *Yellowish.* B. *Lilac.*	Mr. B. Smith	A. *Pale blue.* B. *Darker blue.*
Miss Charlotte	A. *Light dingy yellow.* B. *Lilac.*	Dr. Lee	A. *Whitish.* B. *Light purple.*
Miss Emily	A. *White.* B. *Plum colour.*	Capt. Smyth	A. *White.* B. *Plum-colour purple.*

Now, whatever may be said about instrumental means, tendency of metallic mirrors, weather influence, atmospheric light, or the object's position as to meridian, it is clearly obvious that every property was common to the whole party, and we doubtlessly all meant the same hues. It must be admitted, however, that the star was new to most of the spectators, and, though some of the eyes were surpassingly bright, they had never been drilled among the

is hoped that M. Chevreul's beautiful work on Colours, which has appeared since the above was printed, will yield a proper standard of tints for astrometry, as well as for manufactures, so as to afford an easy and ready reference.

celestials. Further observation, with an achromatic instrument, led me to record Cor Caroli in the Cycle for 1837, A flushed white, and B pale lilac; but, as Sestini found them to be yellow and blue in 1844, I again probed them in 1850, when A struck me as full white and B very pale, but slightly ruddy under that paleness. From the lightness of the tints, this object offers less distinctness than deeper-coloured stars; insomuch that in 1830 Herschel said —"With all attention I could perceive no contrast of colours:" yet at my last inspection in 1855, three observers were unanimous that A appeared to be a pale reddish white, and B lilac, under a magnifying power of 240, and a fair sky. All this shows the urgent necessity of a chromatic scale being drawn up for general adoption; and that, as yet, we are only on the threshold of a very beautiful department of knowledge.

Many persons may think that a mere glance at colours is enough to impress them at once on the memory, and that, without any practice at that sort of remembrance, they can keep any tint in their mind for any length of time; but a more erroneous idea was never entertained. To these unhappy persons greens are greens, and blues blues; for they have never entered even the region of colours, and a whole world of intellectual enjoyment is for ever closed against them. Bring them to the proof of their boasted powers; show them any portion of a landscape; and then place colours before them, and make them put down the various tints from memory, but this a week or two after the scene was witnessed. If hardy enough to attempt the task, every one of their tints will be found in error, and they will only put down one where nature had fifty. Even the painters confess that, though colour may be a low branch of their art, yet it is the most difficult. Only look at the walls of the Royal Academy and see how rarely is a good colourist to be met with, and when he is, how the initiated will gloat over the matchless and magic variety and mellowness of tints, while the uninitiated can see barely more than one, and that to them not noticeably different from the world of common-places beside it. Only look, too, at the characteristics of those painters who *draw* from nature but do not *colour* also from her; who make their

sketches in the open air with pencil or sepia, and fancy colouring to be so simple and so easily remembered, that they may do that afterwards comfortably and at home. Their works are detected wherever they are seen by the poverty of tints, and by the uniformly monotonous colours that are always employed in the same manner: the human mind cannot invent to any extent, but only put together in a novel manner materials collected from the external world. And such materials in colouring can only be impressed on the memory by actual pains-taking and laborious copying and working from nature, by making the tints and applying them in imitation of her. By such training, this branch of memory may be strengthened as well as any other; for we find that the works of artists who adopt this method are always superior in their colouring to those of others, even when they paint from the memory or the imagination. And one of the best colourists that we have ever had in landscape painting was so impressed with the importance of cultivating the memory in this manner, that he used, even in the days of his prosperity and the high prices of his works, to spend much time in the open air making studies in oil, and then, as soon as they were made, tore them up; so that, as the followers of Cortez saw the necessity of conquering when their commander burnt the ships in which they might have made an inglorious retreat, and exerted themselves accordingly,—in the same way not being able to refer, when painting a picture at home, to the sketch made in the open air, he felt himself necessarily obliged to tax his powers of memory, and make them exert themselves to the very utmost.

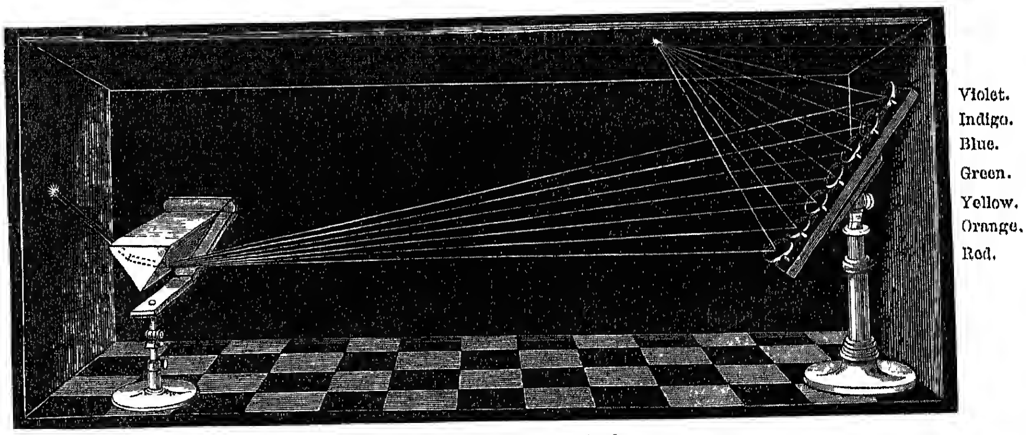
In the early part of this century, it was my good fortune while in Athens to make the acquaintance of the late Signor Gianbattista Lusieri, the eminent landscape-painter engaged by Lord Elgin to work in Greece. This philosophical artist showed me a series of views, proving his gradual improvement through twenty years, by making Nature his model throughout; and he restricted himself, moreover, to the same hour of the day for colour; and some pictures which he was unable to complete before a change of weather, he reserved till the same season of the following year. Hence the perfection of his pencil.

The second, or instrumental method of determining colour, need not be entered upon at much length here, as amateurs are not very likely to practise it; and would be working generally at a great disadvantage compared with any instrument in a public observatory specially devoted to this object. Brightness is everything with this method, and this must be commanded both by elevating the telescope into a high region of the atmosphere, and by adopting the largest possible size of aperture; for, not only must photometrical determinations of the brightness of different sections of the spectra of stars formed by prisms be made, but the black lines in the spectra of each star must also be carefully examined into, as all the transcendant accuracy of modern optics depends on them. This instrumental method of reducing colour to brightness and place, in addition to the exactness of the numerical determination of which it is then capable, would further overcome a most important source of error, and one which has not been touched upon in all that has gone before; and would affect to its fullest extent the method of the "senses," namely, chromatic personal equation.

In the first volume of my Cycle of Celestial Objects, pages 302 and 303, I instanced what is now termed "colour blindness," or the abnormal peculiarity of certain eyes in their being unable to distinguish colours correctly, and yet capable of proper action in every other use of them. Every one knows those violent cases of it where a person cannot distinguish green and red and other such egregious contrasts, and would not admit such a person's observations of colour at all; but it is by no means so generally known as it should be, that a personal equation of greater or less amount exists in every case, and the reason of the faulty colouring of so many artists is, that they really are not aware of many of the refinements of colour: their eyes not perceiving them, their fingers cannot render them. In one of the most intense examples, however, of this chromatic personal equation, although the person could not distinguish so bright a scarlet as the flower of the pomegranate from the genuine green of its leaf, I have had abundant proof that his eye was able to perceive brightness, independent of colour, as acutely, if not much more so, than the

generality of men. It should, however, be observed that there is to the most normal vision a sensible presence of the red element in either violet, lilac, and the various hues indiscriminately termed purple.

These, then, being the advantages of the instrumental method, we may hope that they will not be lost sight of. If it be true that the Government is about to send a large reflector to Australia to observe the southern nebulæ, it might also forward another to a tropical region for observing the planets, and for making chromatic observations of the stars. The Australian telescope will have more work than sufficient with the nebulæ, and the planets with their faint satellites will be low down in the north there while we have them low in the south here; but the equatoreal telescope will have them in its zenith; and it may be elevated on some table-land there far higher into the atmosphere than the Australian one can be; a very important matter where colours rather than brightness are concerned: for a want of the latter may be corrected merely by using a larger aperture; but a distortion of the former, once introduced, is utterly irremediable.



Newtonian experiments on light and colours.

CHAPTER VIII.

THE STORY OF γ VIRGINIS.

The very law which moulds a tear,
And bids it trickle from its source,
That law preserves the Earth a sphere,
And guides the planets in their course.
ROGERS.

In the brief mention which I made of double stars at pages 208-216, it ought, perhaps, to have been noted, that by the measurement of the angle which the meridian makes with a line passing through both components of the object under observation, together with measures of the distance of the stars from each other, the form and nature of the orbit are determined: and of those compound systems which have, to an almost conclusive conviction, been proved to obey the power of gravitation, none has attracted greater attention from the astrometers of the day than γ Virginis.

A detailed history of this very remarkable binary-star, one of the first recognized as a revolving system, was published in my Cycle of Celestial Objects in 1844. But, as the present volume will be before readers who have not seen that work, and in order to bring up the whole matter together to the present time, I shall commence this section with an entire reprint of my former account, with one or two slight explanatory additions:

CCCCLVI. γ VIRGINIS.

DATE 1840	Æ $12^h\ 33^m\ 33^s$	PREC. +	$3^s{\cdot}07$
	DEC. S $0°\ 34'{\cdot}3$	——	S $19''{\cdot}84$

POSITION 77°·9 (w 6)	DISTANCE 1″·6 (w 2)	EPOCH 1831·38
——— 71°·4 (w 5)	——— 1″·2 (w 3)	——— 1832·40
——— 63°·6 (w 7)	——— *not taken*	——— 1833·23
——— 62°·7 (w 8)	——— 1″·3 (w 2)	——— 1833·44
——— 48°·8 (w 6)	——— 1″·0 (w 2)	——— 1834·20
——— 45°·5 (w 5)	——— 0″·8 (w 2)	——— 1834·39
——— 15°·0 (w 5)	——— 0″·5 (w 1)	——— 1835·40
——— *round* (w 9)	——— *round* (w 9)	——— 1836·06
——— *round* (w 9)	——— *round* (w 9)	——— 1836·15
——— *blotty* (w 8)	——— *blotty* (w 5)	——— 1836·25
——— 350°·9 (w 4)	——— *elongated** (w 5)	——— 1836·30
——— 348°·6 (w 4)	——— *elongated* (w 5)	——— 1836·39
——— 265°·4 (w 3)	——— 0″·6 (w 1)	——— 1837·21
——— 235°·7 (w 3)	——— 0″·8 (w 2)	——— 1838·28
——— 217°·2 (w 5)	——— 1″·0 (w 2)	——— 1839·40
——— 192°·8 (w 5)	——— 1″·9 (w 3)	——— 1843·08
——— 191°·6 (w 8)	——— 1″·9 (w 5)	——— 1843·33

A fine binary star, in Virgo's right side, heretofore known as *Porrima* and *Postvarta* by Calendar *savans*. A 4, silvery white; B 4, pale yellow, but, though marked by Piazzi of equal magnitude with A, it has certainly less brilliancy; and the colours are not always of the same intensity, but whether owing to atmospherical or other causes remains undecided. They are followed by a minute star nearly on the parallel, and about 90″ off. With β, δ, and η, it formed the XIIIth Lunar Mansion, and was designated, from its position in the figure, *Záwiyah-al-'auwà*, the corner of the barkers. This most instructive star bears north-west of Spica, and is 15° distant, in the direction between Regulus and γ Leonis, which are already aligned. A very sensible proper motion in space has been detected in A, and there can be no doubt of B's standing on in the same course; the most rigorous comparisons of recent observations afford the following values:—

Piazzi	Æ. — 0″·72	Dec. + 0″·10
Baily	— 0″·50	— 0″·02
Argelander . . .	— 0″·52	— 0″·02
Main	— 0″·55	— 0″·05
Mädler	— 0″·54	+ 0″·01 $\frac{A+B}{2}$

* This was the most puzzling of all my double-star trials; for, so unexpected was the phenomenon, that I gazed long and intently before pronouncing it *round* in the month of January: and it was only on repeated scrutiny that I had an impression that the object was in rather an elongated form in April; which impression was confirmed by the 21st of May. The weights were, however, added to the angle rather to substantiate my own conviction by the senses, than to attest accuracy of measure. About this time I received a letter from Dr. Robinson, of Armagh, informing me that he had no difficulty in elongating γ with Sir James South's large refractor.

It was with much gratification that I watched this very interesting physical object through a considerable portion of its superb ellipse, and I was fortunate enough to attack it during the most critical period of its march. It is rather singular that, brilliant as these two stars are, various occultations of γ Virginis by the Moon have been recorded, without allusion to its being double. So lately as the 20th March, 1780, the phenomenon was watched by nine astronomers; yet at Paris only, on that occasion, is mention made of one star being occulted 10^s before the other. On the 21st January, 1794, the occultation was observed by four astronomers; yet no one mentions duplicity. This is passing strange, because Cassini had, in 1720, perceived and recorded the two stars, noting that the western disappeared 30″ before the other behind the Moon's dark limb, but they emerged nearly together. He could not divide them with a telescope of eleven feet, but with one of sixteen they were well severed, and of equal magnitudes. He watched the immersion, which was oblique, with great care, hoping by refraction or discoloration to detect a lunar atmosphere; but, though the circumstances were favourable, he perceived no symptom. Yet the observation was held to be of importance, for, by enlisting that able astronomer and Bradley, Sir John Herschel considered that he gained some useful points in the orbital departure; and the results of more than a century previous to my measures, may be thus shortly stated:—

Bradley and Pound	Pos. 160°52′	Dist. *caret*	Ep. 1718·20
Cassini II.	*caret*	7″·49	1720·31
Mayer	144°22′	6″·50	1756·00
Herschel I.	130°44′	5″·70	1780·06
Herschel II. and South . .	103°24′	3″·79	1822·25
Struve	98°18′	2″·28	1825·42
Dawes	78°15′	2″·01	1831·33

A mere inspection of the conditions below stated, shows the vast acceleration of the revolving star on approaching its periastre, and the retardation of its getting away again. These are the annual rates of angular retroccession:—

Mayer . . .	0°·43	1756·00
Herschel I. . .	0°·57	1780·06
H. II. and South	0°·64	1822·25
Struve . . .	1°·59	1825·42
Dawes . . .	3°·39	1831·33
Smyth . . .	5°·43	1832·40
——	9°·40	1833·23
——	15°·25	1834·20
Smyth . . .	17° 37	1834·39
—— . . .	30°·20	1835·40
—— . . .	*round*	1836·06
—— . . .	26°·78	1836·39
—— . . .	25°·55	1837·21
—— . . .	22° 01	1839·40
—— . . .	16°·52	1843·08
—— . . .	6°·63	1843·33

As the rigorous observations and computations of this object must be deemed a sort of *experimentum crucis* of the sidereal connected systems, I may be excused for entering into rather fuller details of the detection and establishment of so wonderful an elliptic motion, than I have yet indulged in among the binaries; and it will thereby serve as an example of the method of procedure with those interesting objects.

The various observations were most ably and zealously discussed by Sir John, and treated in a straightforward, geometrical mode, so as to be widely available; as will be seen on consulting the Fifth Volume of the Memoirs of the Royal Astronomical Society. The method is equally novel and ingenious. Assuming that the motions of binary stars are governed by the universal law of gravitation, and that they describe conic sections about their common centre of gravity and about each other, he was bent on relieving their discussion from the analytical difficulties attending a rigorous solution of equations, where the data are uncertain, irregular, and embarrassing. Measures of position were to be the sheet-anchor; for distances, with the exception of the major semi-axis, were peremptorily excluded from any share of consideration in the investigation, because of their notorious looseness and insecurity.

"The process," said he, "by which I propose to accomplish this, is one essentially graphical; by which term I understand, not a mere substitution of geometrical construction and measurement for numerical calculation, but one which has for its object to perform that which no system of calculation can possibly do, by bringing in the aid of the eye and hand to guide the judgment, in a case where judgment only, and not calculation, can be of any avail."

Under the assumption, therefore, that gravitation governs, and one of the components revolves, while the other, though not necessarily in the focus, is at rest, the curve is constructed by means of the angles of position and the corresponding times of observation; and tangents to this curve, at stated intervals, yield the apparent distances at each angle, they being, by the known laws of elliptical motion, equal to the square roots of the apparent angular velocities.

Thus armed, Sir John proceeded with the orbit of γ Virginis. From the above positions and epochs, with interpolated intermediates, a set of polar co-ordinates was derived, and thence, for the apparent ellipse, the following elliptical elements:—

Major semi-axis	5″·862
Position of major semi-axis	67° 20′
Excentricity	0·70332
Maximum of distance	9″·423
Position at the maximum distance	218° 55′
Minimum of distance	0″·514
Position at the minimum distance	1° 15′
Date of next arrival at minimum distance . . .	1834·39
Greatest apparent angular velocity . . .	− 68°·833
Least apparent angular velocity	− 0°·193

The next process was to obtain the elements of the *real* ellipse, and the whole consequent investigation is so succinctly described in the paper alluded to, that any zealous tyro may tread in the

same steps, with a little attention. The results, together with a comparison of the elements and observations up to the period of the computation, and an ephemeris of the system for the years 1832, 1833, 1834, and 1835, were inserted in the Supplement to the Nautical Almanac for 1832. But, finding a discrepancy between the measures then obtained, and the places predicted, Herschel, nothing daunted, again took the field, and recalculated the orbit, as described in the Sixth Volume of the Astronomical Memoirs. In this process, my measures of 1832 and 1833 were included, and the two conclusions stood thus:—

	1831.	1838.
Major semi-axis . . .	11″·830	. 12″·090
Perihelion projected . . .	17° 51′	. 36° 40′
Excentricity	0·88717	. 0·8335
Inclination to plane of the heaven	67° 59′	. 67° 02′
Position of node . . .	87° 50′	. 97° 23′
Mean annual motion . .	—0°·70137	. —0°·57242
Period in tropical years . .	513·28	. 628·90
Perihelion passage . . .	1834·01	. 1834·63

In giving the first part of these remarkable elements to the astronomical world, Sir John said:—

"If they be correct, the latter end of the year 1833, or the beginning of the year 1834, will witness one of the most striking phenomena which sidereal astronomy has yet afforded, viz. the perihelion passage of one star round another, with the immense angular velocity of between 60° and 70° per annum, that is to say, of a degree in five days. As the two stars will then, however, be within little more than half a second of each other, and as they are both large, and nearly equal, none but the very finest telescopes will have any chance of showing this magnificent phenomenon. The prospect, however, of witnessing a visible and measurable change in the state of an object so remote, in a time so short (for, in the mean of a very great number of careful measures with equal stars, a degree can hardly escape observation), may reasonably be expected to call into action the most powerful instrumental means which can be brought to bear on it."*

And Sir John's projected ellipse, drawn through the early alignments, was this—

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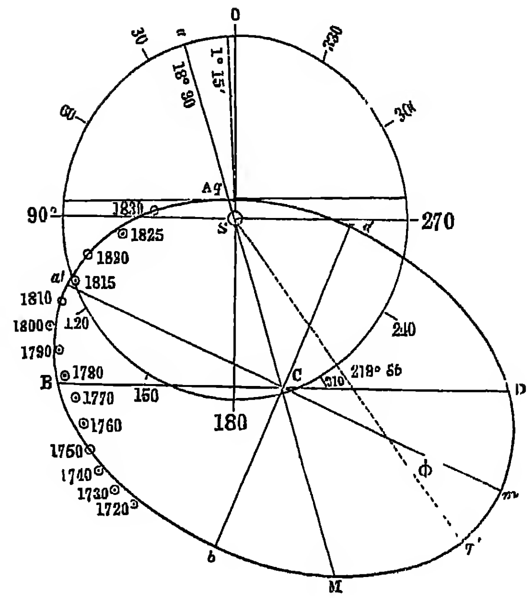
* Here I cannot but append the elements of Mädler's corrected orbit, as they appeared in the *Dorpat Observations* for 1841; the equations of condition being solved by the method of least squares—

Perihelion passage	1836·313
Node	60° 38′
Perihelion from node	78·22
Inclination	24 39
Excentricity	0·86815
Mean Annual Motion	—148′·453
Period	145yrs. 409
Semi-axis Major	3″·402



From the extreme delicacy of so novel a case, all the conditions were not yet met, so that this bold prediction was not circumstantially verified, although it was admirably correct in substance. Whilst rushing towards the nearest point of contact, or shortest distance of the revolving star from its primary, and the proximity became extreme, the field was left, as far as I know, to Sir John Herschel at the Cape of Good Hope, Professor Struve at Dorpat, and myself at Bedford. Our measures afforded unquestionable proofs of the wonderful movement under discussion; yet they certainly exhibited greater discrepancies than might have been expected from the excellence of the instruments employed. But the increased angular velocity which so eccentric a star acquired when gaining its periastre, and the closeness of its junction, rendered the operations extremely difficult: added to which, the brightness of two such stars was sufficient to call forth that disadvantage, arising from the inflection of light, which the wire micrometer labours under, and which interferes in the exact contact between the line and the luminous body.

The accelerating velocity of angular change was thus vigilantly watched, until the commencement of the year 1836, when an unexpected phenomenon took place. Instead of the appulse which a careful projection drawn from the above elements had led me to expect, I was astonished, on gazing at its morning apparition in January, to find it a single star! In fact, whether the real discs were over each other or not, my whole powers, patiently worked from 240 to 1200, could only make the object round. I instantly announced this singular event to my astronomical friends, but the notice was received with less energy than such a case demanded; none of the powerful refractors in this country were pointed to it in time; and it is to be regretted that we had not the benefit of the unexcelled Dorpat telescope's evidence at the critical epoch in question.* This state of

* This was the first instance of a sidereal occultation that has been witnessed, and it still remains so. Several other double-stars, we know, must have performed a similar eclipse, but they were not caught in the fact.

apparent singleness may have existed during the latter part of 1835; for, when I caught it, as may be seen in the observations above, it was very near a change. At length, about the beginning of June, 1836, a letter arrived from Sir John Herschel, addressed to Mr. Baily, wherein he detailed his observations on the single state of this star at the villa of Feldhausen, Cape of Good Hope, in his twenty-foot reflector. Under the date of February 27th, that unwearied astronomer says:—

"γ Virginis, at this time, is to all appearance a single star. I have tormented it under favourable circumstances with the highest powers I can apply to my telescopes, consistently with seeing a well-defined disc, till my patience has been exhausted; and that lately, on several occasions, whenever the definition of the stars generally, in that quarter of the heavens, would allow of observing with any chance of success, but I have not been able to procure any decisive symptom of its consisting of two individuals."

The companion now took such a movement as quite to confute a large predictive diagram which I had constructed, showing that the orbit was extremely elongated, more like a comet's than a planet's; which gave me a suspicion that we had been looking at the ellipse the wrong way. Hereupon I returned to the Herschelian process to obtain the elements of the apparent and the true ellipse with my new measures, but could neither accommodate the period nor arrive at any satisfactory conclusions. When, therefore, M. Mädler's masterly computations appeared in the *Astronomische Nachrichten*, my views were greatly countenanced; but, with a full value for the talent and zeal of that astronomer's process, I was still anxious for Sir John Herschel to return to his own field, and meet the apparently unaccountable informalities which still remained. Having made a request to this effect, he replied:—

"*Maugre* I cannot yet send you any finalities about γ Virginis, yet to prove that I have not been quite idle, I will state one or two general conclusions that a projection of all the observations has led me to, preparatory to exact numerical computation. 1. We are *all wrong*, Mädler and all of us, and it is the early observation of Bradley in 1718 which has misled us. That observation is totally incompatible with *any* reasonable ellipse, and must be absolutely rejected. Had it not been for my respect for that single observation, I should have got very near the true ellipse in my first approximation. 2. The period is short of one hundred and fifty years. My conjecture, antecedent to *any* exact calculation from my projection, is one hundred and forty-three, which is considerably less than the least of Mädler's, and beyond his assigned limits of error. 3. I suspect Mädler's perihelion to be half a year too early, and that the true perihelion passage took place at 1836·6 or thereabouts. We shall get on better now that we have found out the black sheep."

Thus duly authorised, I attacked the orbit again, rejecting, with some regret, Bradley, Pound, Cassini, and Mayer, and assuming H's observations of 1780 as the point of departure. Taking, therefore, the epochs from that date to 1843 for abscissæ, and the observed angles for ordinates, a fresh set of periods was obtained, through which the interpolating curve was led on a very large scale.* From the interpolated positions corresponding to the *assumed dates* between 1780 and 1843,

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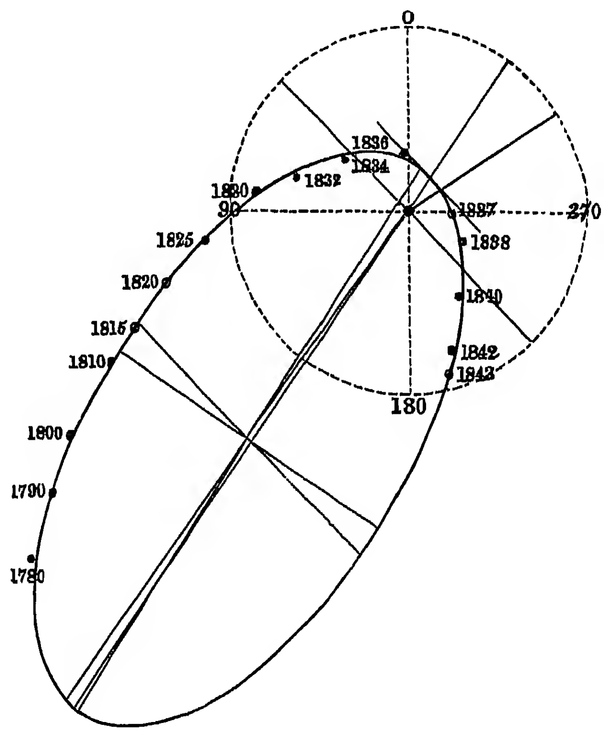
* Sir John Herschel informs me, that he has disused the method of drawing tangents for the angular velocities. The substitute is a closer reading off of the curve, equalising the differences on paper, and thence deducing the angular velocities by first and second differences (if needed); but first will generally suffice.

the intervals being first decennial, then quinquennial, and afterwards more rapid still, the angular velocities were concluded, and by their aid the distances as radii vectores. These positions and distances were laid down from the central star as an origin of polar co-ordinates. Now, though this is a simple and merely graphic process of obtaining the elements of both the apparent and true ellipse, and is liable to shakiness, it undeniably shows the physical fact of a highly-elongated orbit; and several of the conditions prove that, notwithstanding the present anomalous differences, we are arriving near the mark. It is singular how all the determinations of the excentricity have agreed thus:—

	First orbit.	Second orbit.		First orbit.	Second orbit.
Encke . . .	0·890	0·860	Mädler . . .	0·864	0·868
Herschel II. .	0·887	0·834	Smyth . . .	0·883	0·872*

As the ellipse projected by Sir John Herschel, under *all* the epochs, has been given, the reader may like to see the figure produced by the Bedford observations—prolonged from Sir William's first date—which yields a period of about one hundred and eighty years.



* In order to bring this very important condition under one view, I will here subjoin some of the determinations since arrived at, namely—

Henderson . . .	0″·8590	Smyth (*Bedford only*) .	0″·8682
Hind	0″·8566	Herschel (*Cape observation*) .	0″·8795
—— (*Bedford only*) . .	0″·8804	———— (*last*) . .	0″·8860
Smyth . . .	0″·8725	Adams	0″·8796

As the resulting elements, though better, were still unsatisfactory, I was about to take another point of departure and try again, when I received a letter from Sir John Herschel, dated Collingwood, 9th July, 1843, of which the following is an extract:—

"I wrote to you last that I could not make Bradley's observations agree with *any* ellipse consistent with the later observations, and that Mädler's elements, which assume the correctness of that observation, are inadmissible. I have now satisfied myself that this is really the case, and that Mädler's period admits of being yet reduced. But still it is necessary to suppose materially greater errors *in one direction* over the whole interval 1828, 1829, 1830, 1831, than I quite like. The mean of Dawes's and my own measures, however, is on the whole exceedingly well represented in all the critical and puzzling part of the orbit corresponding to 1830—1834 inclusive. Your observations of 1831, 1832, and 1833, offer discordancies of $+2°$, $+2\frac{1}{2}°$, and $+3°$, which are, considering the then considerable closeness of the stars, not more than might well be committed. But Struve's are quite inexplicable; his errors, supposing the orbit correct, run thus:—

1825	1828	1829	1831	1832	1833	1834
$+1°$	$+3°$	$+3\frac{1}{2}°$	$+4\frac{1}{4}°$	$+5°$, $+7°$	$+6°$	$+7\frac{1}{2}°$

after which the deviation ceases.

"On the whole I consider the proofs of gravitation afforded by this star quite satisfactory. It is true that I am forced to admit an error of $-3\frac{1}{4}°$ in my father's measure of 1781, and an error exceeding $2°$ in the same direction in his subsequent mean result for 1803; but when I recollect what sort of micrometer and apparatus he used, I am not disposed to quarrel with these.

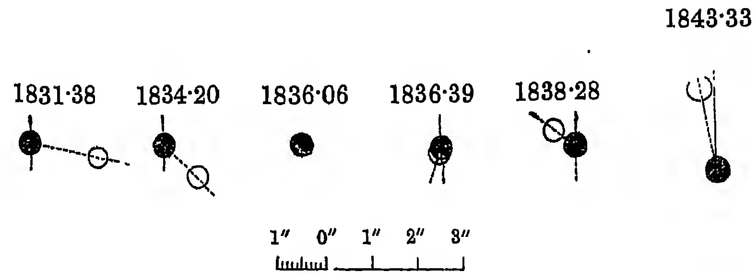
"I am not satisfied with my inclination and node, and there is still a tendency in the curve of the star, if your measures of this year be correct, to run away from its proper course, *to bolt;* which leads me to believe that these elements are not yet so well determined as I hope to get them. Your ellipse from the Bedford observations is a very beautiful one, but I have not yet compared your elements with the observations. I am somewhat surprised at the length of your period, as I find one hundred and twenty-six years represents the mean of all the observations (including Struve's) on the whole well. I have been chiefly attending to improving the *method* as a working one, and I am preparing a paper on the subject, in which the orbit of γ will occur in exemplification. What I aim at is, a *direct process* leading to the separate correction of each element, in place of a turmoil of calculus on the principle of least squares, which in cases of such discordant observations is, if not illusory, at least unnecessarily troublesome."

The inquirers into binary systems will yearn for the coming of this discussion; meantime, to use an expression of Pliny the Younger, I am fortunate in my heliacal rising, since what I have here stated may be of a little interest, before it shall be obscured and eclipsed in Herschel's brighter eminence (see Appendix, No. III).*

One word more. To those who are earnest upon either of these topics, I submit a diagram of

* Three years afterwards, Sir John's elaborate orbit appeared in the noble volume—"Results of Astronomical Observations made during the years 1834-8, at the Cape of Good Hope. Being the completion of a telescopic survey of the whole surface of the visible Heavens, commenced in 1825."

what I saw myself, which may render the above details more evident:—



Such a phenomenon has had more discussers than beholders, so that astute doubts have been flung out of these stars being amenable to gravitation, whether their angular changes are reducible by the laws of elliptical motion, whether the period be a little longer or shorter, and all that. Nay, with such unquestionable instances before the world, and at the very time that admiration was incited in every reflecting mind, a blundering Zoilus, who, had he *flourished* at an earlier age, might have figured at Galileo's trial, was permitted to stain the Church of England Quarterly Review, April, 1837, p. 460, with the following Bœotian attempt at sprightliness:—

" We have forgotten the name of that Sidrophel who lately discovered that the fixed stars were not single stars, but appear in the heavens like soles at Billingsgate, in pairs; while a second astronomer, under the influence of that competition in trade which the political economists tell us is so advantageous to the public, professes to show us, through his superior telescope, that the apparently single stars are really three. Before such wondrous Mandarines of Science, how continually must *homunculi* like ourselves keep in the back-ground, lest we come between the wind and their nobility."

This plural unit must truly be, so far as education and intellect are concerned, the downright veritable *homunculus* he has written himself.

The would-be wit, however, though quite as ignorant, is perhaps less malignant than a fellow reviewer, who must needs meddle with works beyond his ken. This stultified Bavius asserts, that it best suits with the knowledge we possess of our finite understanding, and with the purport and end of our being, to refrain from *silly* speculations which may perplex, but can never satisfy, the mind. He holds it both vain and wicked to attempt to probe the infinity of space, and decries Sir William Herschel's estimate of the magnitude of the nebula in Orion, as a speculation to confuse rather than instruct the mind. This man is susceptible of very great improvement before his opinions command respect. So far from science being daring and proud, as he asserts, there are abundant reasons for it to feel humbled; but the effusion in question shows the proper nursery of those qualities,

For he that has but impudence,
To all things makes a fair pretence.

In strict accuracy, I should here state—though risking a repetition—that the magnitudes and colours above given of the components of γ Virginis, are not

quite satisfactory, inasmuch as I have often been under the impression that the southern star is the brighter of the two; while the tint of the northern one has sometimes bordered upon pale violet, the opposite of yellow. I must now detail the occurrences which have taken place since the above was printed.

While the Cycle was in the press, my much-regretted friend the late Professor Henderson, of the Royal Observatory at Edinburgh, called upon me, and looked over several parts of the manuscript; on which I drew his attention to some of the binary systems, and promised to send him a proof-sheet of the above the moment it should be printed. After forwarding the same, I received a letter dated November 18th, 1843, which is so material to the "Story" from his skilful handling of the subject, that I cannot but reprint it here:—

You will no doubt think me very inattentive for not sooner replying to your letter of 17th October; but when it arrived, I was immersed in work of different kinds: not only that of the Observatory, sufficient when well performed to take up my whole time, but other avocations which had been accumulating from my absence and other causes. But I never lost sight of your communication (for which I am much obliged, as it has forced me to study a subject which I had previously read only in a cursory manner), and expected from day to day to commence its investigation. But it has only been during the last week that, by devoting every spare hour to it, I have satisfied myself regarding it.

The determination of the orbits of double stars from observations presents practical difficulties, in consequence of the great comparative errors to which the observations are liable. The problem is a similar one to that of the orbits of comets deduced from the most rough estimates of their positions, perhaps erroneous to the extent of 20° or 30°. Cases such as these have frequently occurred in the determination of orbits of ancient comets; and it has consequently happened that different investigators have obtained orbits that bear no resemblance to each other.

The oldest observation of the double star γ Virginis that we have, is that of Pound and Bradley in 1718. Sir John Herschel has from it obtained the angle of position 160° 52′. (Memoirs Astronomical Society, vol. v. p. 36.) By trigonometrical calculation I find that in 1718 the great circle joining α and δ Virginis was inclined at an angle of 153° 33′ to the horary circle passing through the middle point between them. If we correct this by the quantity mentioned by him, we obtain 150° 50′ for the angle of position of γ Virginis, observed at that epoch.

The next observation is that of the lunar occultation in 1720, observed by Cassini. The moon was then within less than twenty-four hours of the full, and although the actual immersions at the dark limb were no doubt observed, I do not believe it possible that Cassini saw the actual emersions from the bright limb. Indeed, his words do not bear this meaning, but rather that at a certain moment he saw both stars emerged and parallel to the moon's limb. This of itself implies that the stars were at a small distance from the limb. Besides, the occultation was one of short

duration; consequently, the stars apparently passed near the top or bottom of the moon's disc. In such situation stars that were seen parallel to the moon's limb could not emerge at the same moment. It may be proper to have this occultation recomputed, in order to ascertain whether the calculated relative positions of the two stars satisfy the conditions of their immerging at the two moments indicated by Cassini, and of their being parallel to the moon's limb and at a small distance from it, at the other time mentioned. But it is probable that the unavoidable errors of the Lunar Tables may have too great influence on the result.

When a good stock of observations has been obtained, I believe that, in order to obtain the most probable orbit, we should proceed in a manner similar to that adopted for comets and planets. In the first place, from the requisite number of observations to be selected from the stock, obtain an approximate orbit, to be afterwards corrected so as to represent, as nearly as possible, and within the limits of the probable errors, all the observations. In the first part of the process, Herschel's, Encke's, or Savary's method may be obtained, and distances must be employed, either actually observed, or deduced from the angular velocities; for an attempted solution of the problem at this stage, depending on angles of position alone, would speedily end in a complication of transcendental equations quite unmanageable. If the distances are obtained from the angular velocities, then, according to a remark of Encke, the angles of position from which the velocities are deduced should be taken at intervals of time neither too great nor too small. I should say that we cannot depend on the angular velocity of γ Virginis obtained from Sir William Herschel's observation of 1781; for, not only is it separated from the next of 1802 by too great an interval, but it has no proper one preceding it to give co-operation. I would rather rely on the observed distance of 1781.

When an approximate orbit has been obtained, the differences between the angles of position computed from and observed give the materials for obtaining a set of six normal angles, from which a better orbit may be determined. This is the second part of the process, and it may rest on angles of position alone, if the distances are considered to be unsafe in the circumstances. The method of proceeding which I prefer is that of Mädler, in No. 363 of *Astronomische Nachrichten*. Six equations are formed expressing the relations between the differences of the observed and computed normal places, and the corrections of the elements necessary to be applied in order to make these differences disappear. The solution of these equations gives the required corrections of the elements; but, should they turn out considerable, in which case the values of their co-efficients in the equations may not have been got with sufficient accuracy, it will be adviseable to repeat the process, starting now from the elements corrected. The requisite calculations, if more than one repetition is not necessary, are not laborious, for the calculations are easily made, and great precision need not be effected. In place of Mädler's expressions for the coefficients of $\Delta\, e$ (the correction of the excentricity), $\Delta\, \mu$ (the correction of the mean annual motion), and Δ T (the correction of the time of perihelion passage), I have employed those given by Gauss in the *Theoria Motus Corporum Cœlestium*. Indeed, the calculations are so simple, that in the case of more observations than six, but not too numerous, the method of *minimum* squares may be applied to them all; for if the proper weight can be assigned to each observation depending on its probable error, the orbit to represent best all the observations will be obtained.

I have applied this second part of the process to six selected observations of angles of position of γ Virginis. I assumed for the approximate orbit Mädler's corrected one in the No. of

Astronomische Nachrichten referred to. Several repetitions would have been spared, if I had started from his more correct one given in No. 452 of *Astronomische Nachrichten.* However, I at last obtained the following orbit:

Time of perihelion passage	1836·29
Mean annual motion	2° 30′·59
Ex-centricity	0·8590
Perihelion on orbit	319° 23′
Inclination	23° 5′
Node	70° 48′
Time of revolution	143·44 years.

The following is a comparison between the observed angles of position and those computed in this orbit:

Date.	Angle observed.	Angle computed.	Difference.	Observer.
	° ′	° ′	° ′	
1718·22	150 50	161 16	— 10 26	Pound and Bradley.
1756·29	144 22	141 1	+ 3 21	Mayer.
1781·89	130 44	130 44	0 0	Herschel I.
1803·20	120 19	120 21	— 0 2	"
1822·25	103 24	103 17	+ 0 7	Herschel II. and South.
1825·32	97 24	97 58	— 0 34	Struve and South.
1828·36	91 0	90 26	+ 0 34	Herschel II. and Struve.
1829·30	88 0	87 18	+ 0 42	" "
1830·38	82 5	82 53	— 0 48	Herschel II.
1831·38	74 54	77 41	— 2 47	Smyth.
1832·40	71 24	70 42	+ 0 42	"
1833·34	63 9	61 21	+ 1 48	"
1834·30	47 9	46 27	+ 0 42	"
1835·40	15 0	11 56	+ 3 4	"
1837·21	265 24	266 8	— 0 44	"
1838·28	235 42	235 26	+ 0 16	"
1839·40	217 12	218 23	— 1 11	"
1843·20	192 12	192 38	— 0 26	"

The first two differences are perhaps not greater than might be expected from the modes of observation. The greater difference of 1831 is evidently owing to an error of observation;* while

* Unfortunately it was an error of the press, for Henderson worked from the proof-sheet which had been for-

that of 1835 may be accounted for by the extreme difficulty in the measurement, owing to the closeness of the stars. I have not computed the observation of 1836, as it must be liable to a very considerable error.

These elements appear to be now so correct, that I believe they may be safely employed as the groundwork of future investigations. They differ very slightly from Mädler's last elements given in No. 452 of *Astronomische Nachrichten*, as the following comparison shows:

	Mädler.	Henderson.
Time of revolution	145·409 years	143·44 years.
Mean annual motion . . .	— 148′·453	150 ·59
Time of perihelion passage . .	1836·313	1836·29
Node	60° 37′·6	70° 48′
Inclination	24° 39 ·2	23° 5′
Perihelion	318° 59′·7	319° 23′
Ex-centricity	0·86815	0·8590

The difference of 10° in the position of the node produces scarcely any sensible effect in the computed angles of position. Hence the place of the node will always be subject to uncertainty.

I have not computed the value of the semi-axis major. This is to be done by comparing the observed and computed distances.

The agreement between the observed and computed places is such, that in my opinion it shows satisfactorily that the motions of these stars are subject to the Newtonian law of gravitation.

The result of this investigation has given me great confidence in Mädler's results for other stars. In this instance he has gone the right way to work, and has obtained a good result.

P.S. I omitted to say that the six angles of position from which I computed the orbit are those of 1781, 1803, 1822, 1835, 1837, and 1843. I also omitted to say that Mayer observed a lunar occultation of the two stars, on April 3, 1757. (Mayer's Observations, Part II. p. 18.) Immersions at the dark limb—interval between the two immersions 16 seconds. This observation may yet be calculated. Mädler's last elements, as given in No. 452 of *Astronomische Nachrichten*, are adapted for angles of position numbered 180°, differently from yours, Herschel's, &c.

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warded. My attention was called to it by a correspondent's asking Sir John Philippart, the Editor of that useful publication the United Service Journal, which angle of position he was to trust to—that published in his "Number for December, 1837, or that printed in the Cycle of Celestial Objects in 1844, the one being 77° 54′ and the other 74° 9′?" It was most vexing, not so much that a compositor should mistake a 7 for a four, but that we should not have detected it in correcting the press! Happily all's right in Sir John Herschel's splendid orbit for 1845. The observations of 1836, as I have already remarked, formed a case of extreme difficulty,—for the eyes of one or two friends who were consulted, could only pronounce the object to be single.

From these statements, which, except to astrometers, I fear are somewhat lengthy, it is manifest that, though the several deduced orbits of this system represent, with more or less accuracy, the observations made use of, they differ very materially in other respects, especially in the period of revolution; and that, notwithstanding all the labour expended on the inquiry, considerable doubt remained as to the computed elements. Even under my own operations, the period varied from one hundred and thirty-five to one hundred and ninety-six years! Still a truly important point was carried, and a signal advantage added to physical science, in that the elliptic motion of the binary was completely established. I shall therefore proceed with the further scrutiny of the progressive movements; but, as this "Story" pertains to Bedford and Hartwell, those operations only will be noticed which, so to speak, were personally connectedal—though the high value of the lucid investigations of such philosophers as Encke and Mädler are held in the warmest admiration, and the measures of the Continental astronomers duly respected.

In strictly examining the contending deductions of the several ellipses, I could not but be forcibly struck with the uncertainty of even the apparently best micrometrical measures; and the inter-comparisons above noted, indicate pretty plainly that full confidence cannot yet be placed upon any of them. The importance of the whole question to Sidereal Physics was so obvious, that I was induced to continue my observations as a contribution to the mass of measures which the case still demands; and it becoming palpable that accurate and satisfactory elements are only obtainable by the unremitting exertions of various practical astronomers, I addressed a letter to the Royal Astronomical Society in May 1845, showing the necessity of following up γ, because that remarkable system promises to be comparatively to double-stars, what Halley's comet is among that class of bodies. The mean results of the measures which I then handed in, gave the following places—

POSITION 185° 23′·3 (*w* 6) DISTANCE 2″·10 (*w* 4) EPOCH 1845·34

Soon after this was published, namely in July of the same year, I had

the gratification of receiving a letter from my indefatigable friend J. R. Hind, inclosing the following orbital elements of γ Virginis:—

Perihelion passage 1836·228.

Perihelion on the orbit	319° 46′·1
Node	78° 28′·4
Inclination	25° 14′·1

Eccentricity 0·85661 $\therefore \phi = 58° 56'·3$

Mean annual motion — 152′·871

Period 141yrs297.

For the calculation of the angles of position in this orbit, we have:—

$$u - [3·46905].\ \sin.\ u = [2·18433]\ (1836·228 - t)$$
$$\tan.\ \tfrac{1}{2}\ v = [0·55609].\ \tan.\ \tfrac{1}{2}\ u$$
$$\tan.\ (\theta - 78°\ 28'·4) = [9·95644]\ \tan.\ (v - 118°\ 42'·3$$

u being expressed in minutes.

The epochs employed and the errors of my elements are as follows:—

		θ observed.	Comp.—observed.
HERSCHEL I.	1781·89	130° 44′·0	—4′·8
———	1803·20	120° 19′·0	—1′·7
HERSCHEL II. and SOUTH .	1822·25	103° 24′·0	+3′·5
DAWES	1831·33	78° 15′·0	+8′·8
Captain SMYTH	1838·28	235° 42′·0	+9′·8
———	1845·34	185° 23′·3	—5′·6

The sums of the squares of the errors in my orbit = 243·0
In Mr. HENDERSON'S orbit = 589·0.

The conditions thus kindly furnished, indicated that the various computations were now approaching something bordering on unanimity in the periodic time of perihelion *(periaster ?)*, the last point to arrange; while that very important element, the ex-centricity, was evidently near the mark. So far so good; still, in order to aid the ultimate settlement of such a desideratum to the utmost, I repaired to my post during the two following apparitions: and these were the obtained results:—

POSITION, 182° 58′ (*w* 7); DISTANCE, 2″·6 (*w* 4); EPOCH, 1846·39.
——— 181° 52′ (*w* 6); ——— 2″·6 (*w* 5); ——— 1847·41.

Although what had been achieved in several quarters would now admit of an interval from work taking place, I again angled to get hold of a plausible period; for that element had hitherto proved so precarious, as seemingly to carry an inherent uncertainty into the problem. Throughout the proceedings, the conformity of the elliptic motion to the great law of gravity is assumed; and, in order to arrive at speedy conclusions, Herschel's graphic method of drawing tangents to an interpolating curve struck me as being at least equal to our present power of observing. To be sure Sir John had, as I have said, abandoned the use of tangents: and he recommended me an easy and simple numerical process, which does away with the errors incident to the laying down of angles, and the problem becomes merely one of conic sections. His letter—dated Collingwood, 20th April, 1847—is so truly interesting in an astrometrical light, that I cannot but take the liberty of here inserting it:—

First and foremost let me mention that in the sheet I sent you about γ Virginis, page 299, there is a vile erratum—the semi-axis is stated 9″·69 (owing to an unreduced value of a having slipped into the copy instead of the reduced one). The real value is 3″·58, which agrees well with yours; 9″·69 must have startled you, as it did me when I came to refer to it.

Next let me observe that your new orbit, I mean the first you give in your note, does not so very far deviate from mine—for you make your $\lambda + \Omega = 269\ 17 + 48\ 56 = 318\ 13$

and I make it . . . . $313\ 45 + \ 5\ 33 - 319\ 18$

and both (the inclinations being small) are not far in their value from π, which in your orbit is 319° 46′, and in mine comes out by formula tan. $(\pi - \Omega) = \cos.\ \gamma\ \tan.\ \lambda$

$$\pi = 321°\ 48'$$

and I hold it for certain that this value cannot well be more than a degree or two wrong. Any how $\lambda + \Omega$ has a remarkable fixity.—See how the orbits run in this respect—

Henderson's $\Omega + \lambda =$	319° 23′
Hind's	319° 46′
My last	319° 18′
Mädler's Ast. N. No. 452	319° 0′
Mädler (letter of Sep. 29, 1845)	320° 20′
Your first orbit in this note	318° 13′
Your second do.	319° 10′*

* My position of perihelion in the orbit which follows = 319° 46′, and is the element quoted by Sir John in the second paragraph of this letter.

Really this is very remarkable. Quite as much so as the exceeding correspondence of all the excentricities.

The real difficulty is and always will be about ☊ and γ.

When the inclination γ is under 30° or thereabouts, this difficulty will always arise. In fact if γ be very small, both γ and ☊ become indeterminate.

The provoking thing is the excessive latitude of P. And on this point I question if we shall come to any correct conclusion, till a revolution has been nearly accomplished.

Mr. Dawes sends me as the results of his measures with the Munich telescope:—

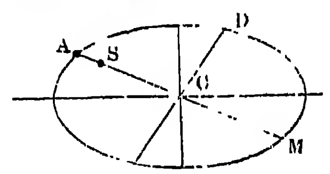
1847·27, θ=182° 2′. Dist.=2″·63.

Capt. Jacob's measures, in a former note of yours, give:—

1846·32—θ=182° 12′. Dist.=2″·89.
mean 1846·795—θ=182° 7′. 2″·76.

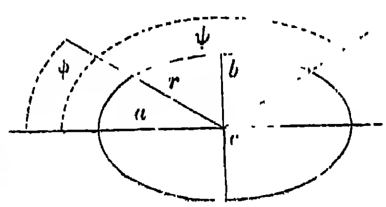
which projected on my chart falls almost to a nicety upon my interpolating curve. Still however Dawes's recent measures, agreeing with Mädler's, give positions less advanced than my curve would indicate, from which it would seem that even 182·12 years (my P.) is too short. But that I can hardly believe.

I now absolutely reject drawing any tangents at all. As you very rightly remark, when the tangent is drawn at the extremity of the major axis projected as situated in the present case, a trifling error of graphical manipulation spoils all. The numerical process is easy and simple. The problem is this:—Given the semi-axis major and minor of an ellipse, and the length of any one of its semi-diameters.



Required, first, the length of the semi-diameter conjugate to it; secondly, the angles which these two semi-diameters make with the axis and with each other.

This is an easy problem of conic sections, and it does away with all errors of the graphical laying down of angles, substituting measurements of length which are much more accurate.



Given . . . . a, b, r,
Required . . . . r^1, ϕ, ψ,
and ∴ . . . . $\psi-\phi$.

1st. $r^2+r'^2=a^2+b^2$ ∴ $r^1=\sqrt{a^2+b^2-r^2}$

2d. $\cos.\ \phi=\frac{a}{r}\sqrt{\frac{r^2-b^2}{a^2-b^2}}$

3d. $\tan.\ \psi=\frac{a^2-b^2}{a^2}\text{cotan.}\ \phi$.

which by-the-bye are not ugly formulæ, and of exceedingly easy calculation. This relation $\tan.\ \phi \times \tan.\ \psi$ = constant, I don't remember to have seen in treatises on conic sections. It is very useful.

But, as my main view was to find what great nicety would effect in the geometrical inspection of the case, as still being, under all its imperfections, an inviting method for amateurs, I again sought the radii vectores through the angular velocities, although commanding the advantage of having direct measurements of distance at hand; and a steady course was pursued, in face of the hazard as to whether the points of observation fell at all remote or irregular. To many, except the class I am addressing, the following details of my orbit for 1847 may prove too wordy, but it can readily be "skipped."

Notwithstanding the rebukes upon Bradley's noted observation of 1718, I thought that the curve might be brought in so as to afford points for an apparently reasonable ellipse, by adopting my last drawing (*ut supra*), instead of that used by Sir John in his first paper. As, however, the paucity of observations about that date allows such latitude in the manner of drawing the curve, I did not, when passing an ellipse through the deduced points, endeavour so much to meet them as those in the more authenticated part. Different circumstances, it is true, will modify Bradley's point to different uses, but, as it hardly satisfies the requirements of the orbit, I shall not introduce it into future argument: it is a remarkable observation, certainly, but at best is merely an angle of position obtained by an arbitrary allineation of the two components of γ with α and δ Virginis.

Proceeding as before stated, and laying down from the assumed central star the positions interpolated, with the several distances calculated from the resultant curve, I found that the figure which best met them all bore the following elements for the apparent ellipse:—

Major semi-axis	$3''\cdot0697$
Minor do.	$1''\cdot7643$
Position of major semi-axis	140°
Ex-centricity	0·8143
Greatest apparent distance	$5''\cdot748$
Position at do.	139° 40′
Least apparent distance	0·393
Position at do.	319° 50′

To arrive through these at the elements of the real ellipse, I brought the tangential method to a severe trial; and it became sufficiently evident that the slightest error of projection might mar the problem, where, as in the instance before us, the projected and apparent major-axis were only 14′ apart. Indeed it seemed difficult to make sure of not drawing the tangent incorrectly by a degree or two; and the difference of a few minutes only in the inclination of this tangent completely alters all the other elements when using the formulæ recommended. Few desirable cases are without obstacles: under all the difficulties of the present one, the attempt at conquest was made, but as some of the values would not adapt themselves to the instance—for they brought out the real minor semi-axis less than the apparent one, which is impossible—I was at the trouble of obtaining the real semi-axis of their inclinations by the more operose method of computing the major semi-axis from the minor one and the ex-centricity, both of which were given by measurement; and from thence the case of least possible inclination was deduced. For the periodic time—although perhaps it may be easier to calculate than to weigh the area—I cut out the orbit, and parts of it, as before; not only using the time as given by the two extreme observations, but also as given by other intermediate ones, and then taking the extremes. Thus the whole of the elements of the real ellipse were obtained: and here they are—

Major semi-axis	$a = 3''{\cdot}61736$
Minor do.	$b = 1''{\cdot}7673$
Excentricity	$e = 0.87253$
Position of perihelion . . .	$\pi = 319^\circ\ 46'$
Position of node	☊ $= 41^\circ\ 40'$
Inclination of orbit . . .	$\gamma = 31^\circ\ 57'$
Angle between nodes and apsides .	$\lambda = 269^\circ\ 17'\ 30''$
Perihelion passage . . .	$\tau = 1836{\cdot}2$ A.D.
Periodic time	$P = 148{\cdot}2$ years.

I was somewhat disappointed on finding that, after all my care, the period should differ by forty years from my former attempts, though a little con-

soled on recollecting the causes of uncertainty in this very ticklish condition; nor was I pleased with another point or two. Yet, in order to try the value of this set of elements, and form an ephemeris of the object, I cut card-board sectors of the graphic projection to find the epoch and distance at any particular angle, and weighed them; thereby adroitly avoiding the solution of the transcendental equations, which are necessary for comparing these elements with the observations. The segments were cut,* not to such angles as had been observed, but to such as appeared to have been correct angles at the time of observation; and thus they ran:—

Angle = 160° 52′	A.D. 1720·23	Angle = 320°·0	A.D. 1836·15	*min. dist.*
159°·8	1721·67	233°·6	1837·96	
142°·0	1756·95	191°·5	1843·54	
120°·2	1804·04	182°·6	1846·92	
102°·9	1822·18	182°·0	1847·25	*Predictions.*
97°·4	1825·25	178°·0	1849·37	
83°·5	1830·14	175°·0	1851·39	
78°·1	1831·30	172°·0	1853·69	
62°·8	1833·23	169°·0	1856·94	

These results appeared to represent the observations pretty fairly as to the angle, and the distances promised to do equally well, but they were not compared, because the error in Bradley's observation was accidentally unconsidered; yet we may judge roughly, that the correction suggested of 10° would increase the period and diminish the inclination. As there were now some symptoms of coming up with the chase, I resolved to trim a little closer, and attempt to form an orbit from the Bedford measures—1831 to 1847—only. I was quite aware that in the present state of the question this attempt might not be deemed altogether legitimate, since the problem is still held to require the

* I have had very powerful aid for carrying the weighing process to the greatest nicety of which it is capable; having been furnished with large sheets of card-board made as nearly homogeneous as possible, by Mr. Dickinson, of Abbot's Hill, F.R.S. the well-known and skilful proprietor of the extensive paper-mills near Hemel-Hempstead: and Mrs. Somerville kindly presented me with a singularly delicate balance, made by Robinson, of Devonshire Street, under the immediate eye of the late Dr. Wollaston.

united products of all the extra-meridian observatories: but, being desirous of ascertaining where we should be had I been the only watcher of the phenomena, the experiment was made without the least intention of undervaluing or slighting the observations of others. This scheme gave for the apparent ellipse a major semi-axis = 2″·902, with its angle of position = 141° 30′, and a minor semi-axis of 1″·698; the real ellipse being—

Major semi-axis	$a = 3''\cdot345$
Ex-centricity	$e = 0\cdot8682$
Perihelion	$\pi = 140° 30'$
Position of node	$\Omega = 40° 44'$
Inclination of orbit	$\gamma = 30° 36'$
Distance of Perihelion from node on the plane of the orbit	$\lambda = 278° 26'$
Date of perihelion	$\tau = 1836\cdot5$
Periodic time	P = 135 years!

Another more ex-centric ellipse which was protracted on a larger scale with a period about ten years longer would have satisfied the observations almost equally well with this; but the above set of elements meet the older ones more fairly. Still the period of one hundred and thirty-five years is certainly curious, as being within all the former limits, while τ is in excess. Such an unexpected result indicated something not yet in its proper place; and it became a question whether we are, or are not, to admit the reality of the star's apparent singleness in 1836, as observed by Sir John Herschel and myself. If it be admitted, it becomes a matter of doubt whether any orbit can be found to represent all the data, without supposing some extraneous perturbations about the time of perihelion.*

While pondering upon these matters, I received the welcome present of Sir John Herschel's truly important volume of Cape Observations, in which the re-investigation of the orbit of γ Virginis, by a strictly careful examination

* On this crucial point—extraneous perturbations—see the letter which Captain Jacob (*post*) wrote to me from Madras: and for the perplexity of some of my friends respecting the disturbing cause in the movements of 70 Ophiuchi, see the Cycle of Celestial Objects (II. page 407).

of all the recorded measures, forms a very interesting point. There the whole details are so ably sifted, that no one interested in the matter should rest till he has read it: but to others it may be told, that Sir John has now abandoned the large elliptical orbit which seemed to be necessary to include the observations of Bradley and Mayer; and, having rejected these data, has adopted the angles of position taken for the epoch of 1781·89 when it was first measured by his father, and that of 1845, which was measured by myself, as the extreme epochs. This able and indefatigable astronomer had already told me that the change of principle in the process he had recommended, as before referred to, mainly consisted in using the angle θ as the independent variable, and interpolating and calculating t (the time) from it; by which means the solution of the transcendental equation, $nt = u - e. \sin. u$, is avoided, as u is given by $\tan. \frac{1}{2} u = \sqrt{\frac{1+e}{1-e}} . \tan. \frac{1}{2} \theta$. The obtained elements of this searching and very masterly investigation of all the observations are—

Excentricity	$e = 0·87952$
Inclination to the plane of projection .	$\gamma = 23° 35' 40''$
Position of ascending node . . .	$\Omega = 5° 33'$
Angular distance of perihelion from node on the plane of the orbit, or true angle between the lines of nodes and apsides .	$\lambda = 313° 45'$
Epoch of perihelion passage . .	$\tau =$ A.D. 1836·43
Periodic time	$P =$ years 182·12

Sir John judiciously sums up by saying,—"Comparing the orbits which seem entitled to most reliance, it appears certain that the eccentricity lies between 0·855 and 0·880, the inclination between 23° and 27°, the perihelion epoch between 1836·20 and 1836·45, and the period between one hundred and forty and one hundred and ninety years."

The paramount interest of this elaborate orbit, both to the present and future story of γ Virginis, is so clearly obvious, that I resolved to give it entire in the Appendix, where the reader will find it under No. 111.

In the meantime γ Virginis had again become an object of very easy

measurement; and the results now obtainable are likely to be as trustworthy as our present micrometric power will admit of. And indeed it is in good keeping, being closely attended to in several parts of Europe and America; while the zealous Captain Jacob has not allowed it to pass unobserved in India. These are my own additional data—

POSITION	179° 31′ (w 8);	DISTANCE	2″·8 (w 5);	EPOCH	1848·36
———	178° 37′ (w 8);	———	2″·9 (w 6);	———	1849·31
———	177° 02′ (w 7);	———	2″·9 (w 5);	———	1850·28.

Having thus watched this extraordinary system for upwards of twenty years, and observed it pass through so important a phase of its entire revolution that I have actually measured the companion in each quadrant of the circle, I prepared to close my telescopic observations upon it with the angle and distance of 1850. But, as the occasion was one of great interest to me, I applied to several friends possessed of powerful instrumental means, begging of them to obtain sets of measures and oblige me with the results, in order to afford evidence, that the opinion I had expressed of all present observations approximating near to each other, might be tested. Urania, however, does not always command her votaries properly; and, at the due season of apparition, those scenes redolent of mortality and gas were unhappily preferred by one or two to the fresh air of the observatory. Moreover, Mr. Hind had overworked himself, and was unwell; while the instrument of one of our very best astrometers, the Rev. Mr. Dawes, was lying dismounted. Two of my correspondents responded cheerfully to the call, and I append their fruits:—those of Mr. M. I. Johnson—from a mean of six evenings—were taken with the new Oxford Heliometer, although it was then but just mounted, and the manipulation of it hardly attained; and those of Mr. Arthur Kett Barclay were observed under powers of 277 and 300, with a fine six-inch refractor of above eight feet focal length, on a German mounting, driven by clock-work—

Johnson . .	∠ of Position 179° 45′;	Distance 2″·76;	Epoch	1850·20.
Barclay .	——∠—— 178° 07′;	——— 3″·09;	——	1850·32.

While gazing at the small companion which follows γ nearly on the parallel, I have more than once been struck with a sensation that another minute star was in the field; but even the idea was evanescent. However, to settle the question indisputably, I requested of both Lord Rosse and Mr. Lassell to search the spot for me, with their gigantic and powerful reflectors: and the result of their kind examination is, that no star exists between the binary pair and the little follower.

On again weighing an orbit from my own measures brought up to 1850, with special care in leading the curve through the epochs of the projection, I obtained a period of one hundred and sixty-eight years. From the sectors produced, the prediction angle for $1851 \cdot 5 = 176°$, to test which I again applied for comparing observations to some of my astrometric friends, whose replies, being very illustrative of this portion of the Story, I subjoin. The first in date (Wateringbury, 25th March, 1851) is from the Rev. W. R. Dawes:—

I fear you will think I have quite forgotten your request that I would send you a measure of γ Virginis as soon as practicable. You will see however that this is not the case: but, at the same time, I must put your faith in my veracity to the stretch by affirming that the measures now sent are the first, and the only ones, I have been able to procure this year! After I saw you a tremendous cold confined me for some time to the house; and being accompanied by unusual oppression on the chest, I found it necessary to be cautious of long exposure to the cold or night air. Then a series of bad starlight nights occurred, several of which clouded over early; and two or three tantalized me with a view of γ Virginis, more like a great oval nebula, or rather a silk-worm's cocoon, than any object measureable as a double star. I really thought it would be insulting to put on record, by attempted measurements, the appearance of the elegant creature when suffering from such an awful shivering fit as produced a horrible deformity, completely amalgamating all her features into one shapeless mass. So I very unwillingly closed my eyelids (shutters) against so sad a spectacle. Last Friday night, however, after watching the formation and partial dispersion of hazy clouds for several hours—feeling assured that the air ought to be fine if the cloud would but take itself off—while looking at γ Leonis, I was amazed and delighted with a rather sudden tranquillizing of the image, which left nothing to be desired in the way of definition. I was using at the time a wheel of double convex lenses, and was aware I had got a pretty high power on; but having been turning the wheel about, I did not know which lens was in use. I thought, however, it was the highest but one, No. 5, power 641; so exquisite were the discs, with a single almost quiet ring round each, that I thought I would give the wheel one more click, and try No. 6, power 903. When, behold, on giving the aforesaid click, I found I was already using 903! I instantly detached the eyepiece and applied the micrometer; and being duly pinned and

loaded with power 500, levelled at the other γ; when, lo! before I could get her fairly in focus, a cloud began to form over her; and, being determined not to be cheated if I could help it, I employed about an hour in getting three angles, and four distances, as the star appeared by glimpses in the chinks between large masses of cloud. It was worth while, however, for the results are good; and therefore I have pleasure in offering them, as follows:—

1851·217, Position = 176°·58; Distance = 2″·982.

Though I have recorded, or rather given you, the angle as south following, the northern star was noticed as being certainly the smaller: which I observed to be the case also throughout the apparition of 1848.

The next is from Lord Wrottesley (Wrottesley Hall, 18th April, 1851):—

Having at last succeeded in getting ten more very capital measures of γ Virginis on a very favourable night, I no longer delay sending the results to you, and, as I promised, I send you the original entries of the observations, as read off from the instrument. The general result is,—

ANGLE	= 175° 55′
with probable error . .	= 3′·65
and weight	= 75
DISTANCE	= 2″·911
with probable error . .	= 0″·015
and weight	= 107
MEAN EPOCH	= 1851·172

The probable errors and weights are computed by the formulæ,

$$P^2 \quad . \quad . \quad . \quad . \quad . \quad = \frac{\cdot 4549}{n(n-1)} \times \Sigma \,.\, e^2$$

$$\text{and weight} \quad . \quad . \quad = \frac{1}{P^2}$$

The third letter is from Mr. Isaac Fletcher (Tarn Bank, Cockermouth, 17th June, 1851), describing the instrumental means with which he obtained the subjoined measurements—

1851·401, POSITION = 175° 58′ DISTANCE = 3″·047.

I have much pleasure in embracing the earliest opportunity to reply to thy note of the 14th instant.

The telescope employed in procuring the measures of γ Virginis, which appear in the May

No. of the Notices, and which I employ in all my double-star operations, is an achromatic refractor of about 6 feet focus, with a clear aperture of 4·14 inches.

The mounting is almost precisely similar to that of thy $8\frac{1}{2}$ foot equatoreal, illustrated and described in the "Cycle."

The polar axis is 9 feet long, 9 inches square at the middle, and 7 inches square at the ends. It is made of 4 planks of mahogany, screwed together and bound internally. The hour and declination circles are each 20 inches in diameter, and read by verniers, the former to 2^s of time and the latter to 10″ of space.

The defining power of the object-glass is first rate; it brings out the 5th star in the great nebula of Orion, and shows with distinctness the companions of ζ Herculis and δ Cygni. The power employed in measuring γ Virginis is 300.

And, finally, Mr. J. F. Miller, of Whitehaven, having watched γ Virginis into daylight, and having gained the last measures of her then apparition, favoured me with the following communication, dated July 19th, 1851 :—

I have not long put my instrument to micrometer-work, and being desirous to get an observation of γ Virginis before it is lost for the season, I have secured two sets of measurements, which I send you, as I know you are particularly interested in this remarkable star. I do not suppose you will consider them of any value, but you will see how they correspond with those of other observers. The observations in position I believe may be relied on as trustworthy.

Micrometrical measures of γ Virginis taken at the Observatory, Whitehaven, with a six-foot achromatic equatorial, 4·1 in. aperture, driven by clock-work. Micrometer by Simms; position circle 5 inches diameter:—

	Observations.	Position.	Distance.	Observation.
1851, June 22 .	8	175.50	,,	,,
1851, June 26 .	7	175·58	3″·040	8

The telescope is exactly the same size as Mr. I. Fletcher's, Tarn Bank, Cockermouth; the two have been tried together side by side, and no difference in performance was perceptible. I readily saw the *comes* of ζ Herculis on the 26th June. λ Ophiuchi is comparatively easy even at this season. My instrument is mounted precisely like that of Ross in the Great Exhibition.

Meantime Mr. Hind has kindly consented to undertake another orbit from my observations only, by a proceeding modified from his last: and it was somewhat satisfactory to have the discoverer of ten new planets at work in cooperation. On the 8th of April, 1851, he wrote to me in these gratifying terms:— "I forward the results at which I have just arrived, from a discussion of your

observations of γ Virginis; but, not having yet been able to compare them fully with the data, I must ask you to regard them with some indulgence. For my own part, I am really astonished at the very close resemblance between these elements and those obtained from a discussion of all the observations, including Bradley's and the elder Herschel's. It speaks volumes for your admirable measures, and is the more remarkable, as I have not used any others or looked at any others for this purpose; in which, I believe, I was carrying out your wish."

Here, to my great satisfaction, I found a retrograde annual motion of 2°·0987 at the epoch, coinciding exactly with elemental deductions; which, with certain other orbital similarities, led to the conclusion that we had now positively passed the threshold, and gained an insight of the true elements. As I had the pleasure of seeing Mr. Hind soon after he wrote, we talked over the case, and considered that it constituted a fact which some of the stellar inquirers would like to be acquainted with; and, as these sheets are only destined to a limited circulation, we agreed upon the eligibility of his communicating it to the Royal Astronomical Society, at their meeting on the following evening, April 11th. This is the statement, as copied from the Minutes, and inserted in vol. xi. No. 5, of the Society's Monthly Notices:—

On the Elements of the Binary Star γ Virginis, resulting from a discussion of the Measures taken by Capt. W. H. SMYTH, R.N. between the years 1831 and 1850. By J. R. HIND, Esq. Foreign Secretary.

It is well known to the members of the Royal Astronomical Society, that amongst the stars observed for the Bedford Cycle by Captain Smyth, the interesting binary system γ Virginis occupied a prominent place. During the period included by the Bedford measures, a very critical and important part of the orbit was passed over, and great pains were bestowed upon the observations to render them as accurate as the nature of the object would permit. In 1831 the component stars were separated about $1''\frac{1}{2}$, the smaller one being situate in the north following quadrant about 12° above the parallel of declination. From this position it was watched by Captain Smyth during its passage through the same quadrant, the central distance diminishing each year, until in the early part of the year 1836 the star was pronounced single under the best atmospheric conditions. Before the close of the spring the Bedford telescope again afforded indications of duplicity, and two nights' observations showed that the companion had just completed a fourth part of its orbit, its

position being now 12° north preceding the principal star. In 1837 a further change of no less than 83° in the same direction had taken place, the *comes* lying in the angle 265° at a distance of rather more than half a second of arc from the primary. In 1847 Captain Smyth found it still on the preceding side, but at a central distance of 2″·6, and his observations early in the year 1848 showed that it had just passed the vertical point, the measures yielding an angle of 179°·5; and at the date of the last observations in 1850, the angle had further diminished to 177°·1.

It thus appears that the whole series of measures taken with the Bedford telescope include a change in the position of the companion-star of 260°, or nearly three-fourths of a revolution, extending, as before remarked, over a very important part of the orbit. The question, therefore, naturally suggests itself, whether an investigation of the elements from this series of measures, to the exclusion of all others, might not be one of some value, as showing, by comparison with elements founded upon the whole of the measures of other astronomers, including the valuable alignments of Bradley and Pound in 1718 and 1720. what kind of dependence we may place upon elements for other binary systems computed under similar circumstances, where, during the interval between the earliest and latest observations, a portion only of the ellipse has been described, which is to be traced chiefly from the measures of one observer.

In the present instance the investigation promised to lead to results of especial interest; the measures of γ Virginis, published in the Bedford Cycle, were taken by one of the most experienced observers of the present day in this particular department of astronomy, were made throughout with the same instrument, and under the most favourable conditions as regards the state of the atmosphere, the powers employed, &c. With data derived under these promising circumstances, an orbit fairly approximating to the true one (or to that which we have strongest reason to rely upon) might be expected as the result of their discussion; but I confess I had no idea that this series of measures (leaving untouched, as it does, the more distant part of the apparent ellipse when observations are made with comparative facility and proportionally greater accuracy,) would produce a set of elements bearing such close resemblance to those of Sir John Herschel, which, having been calculated upon the whole course of observations, on a method possessing peculiar recommendations, we may fairly presume to be the most exact system at present in the hands of astronomers. For the sake of immediate comparison the two orbits are subjoined together:—

ELEMENTS OF γ VIRGINIS.

	I. From the Observations of Capt. W. H. Smyth.	II. Sir John Herschel's last Elements.
Perihelion passage	1836·40	1836·39
Position at perihelion	323° 50′	322° 12′
Ascending node	20° 34′	28° 42′
Inclination to plane of projection	27° 23′	30° 39′
Angle between the lines of apsides and nodes upon the orbit	300° 13′	290° 30′
Excentricity	0·8804	0·8860
Period of revolution	171^{yrs}·54	183^{yrs}.14

Motion in the orbit—*retrograde*.

In comparing these orbits, it must be borne in mind that in the present case an alteration in the position of the node of 10° has but very little influence upon the computed angles of position; and for this reason the node and the angle between the lines of nodes and apsides cannot be exactly ascertained. The agreement between the other elements is, I think, very remarkable; and, as regards the period, the observations of Capt. Smyth furnish us with the time of revolution of the companion-sun round the other, differing less than twelve years from that which, under existing circumstances, may be regarded as the true one.

After having heard this interesting communication read to the Meeting, on returning homewards in company with my friend Mr. J. C. Adams, of Neptunian celebrity, we entered into a discussion upon the several orbits of γ Virginis; and I was not scrupulous in pressing him into the question, since I feel fully persuaded that sidereal physics must one day become of the highest paramount interest to transcendental investigators; and we are well aware of the advantages of combination. After a few preliminaries, I found him "nothing loath" in the matter, and he undertook to attack the problem by a method of his own. The result I had the gratification to receive, under date of the 29th of June, 1851; and was in hopes the details, with a full account of them, would have been given at the next meeting of the Royal Astronomical Society. Meantime the following extract from his letter shall form the satisfactory conclusion of this lengthy "Story:"—

I have great pleasure in sending you the results which I have obtained respecting the orbit of γ Virginis, and I feel the more indebted to you for having called my attention to the subject, inasmuch as the problem of determining the orbits of double stars is one with which I had previously only a theoretical acquaintance. The orbit given by Sir John Herschel in the Results of his Cape Observations, was taken as the basis of the calculations, and equations of condition for the correction of the elements were formed by comparing certain selected angles of position deduced from observation with the values calculated by means of Sir John Herschel's elements.

The positions employed are those given by Bradley's observation in 1718, Sir William Herschel's observations in 1781 and 1803, a normal position for 1825 deduced from the observations of 1822, 1825, and 1828, one for 1833 from the observations of 1832, 1833, and 1834, another for 1839 from the observations of 1838, 1839, and 1840, and, lastly, a normal position for 1848 from the observations of 1846, 1847, 1848, 1849, and 1850. The number of these positions being greater by one than that absolutely necessary for the determination of the elements, I at first omitted the equation of condition for 1718, and solved the remaining ones in such a manner as to show the effect which would be produced in each of the elements by a small given change in any one of the observed angles of position. The result proved that the elements would be greatly affected by

small errors in the observed positions for 1781 and 1803, and I therefore called in the observation of 1718 to the rescue, and solved the equations anew, supposing the positions for 1825, 1833, 1839, and 1848 to be correct, and distributing the errors among the other three, according to the rules supplied by the method of least squares, giving double weight to the observations of 1781 and 1803.

The following are the resulting elements:—

Inclination of the orbit to the plane of projection .	25° 27′
Position of the node	34° 45′
Distance of perihelion from the node . . .	284° 53′
Angle of ex-centricity	61° 36′
or ex-centricity	0·87964
Perihelion passage	1836·34
Period	174yrs137

The following table shows the differences between the observed positions and those calculated from the above elements:

Epoch.	Observed Position.	Calculated Position.	Difference.
1718·22	150° 52′	151° 3′	— 11′
1781·89	130° 44′	130° 29′	+ 15′
1803·20	120° 15′	120° 43′	— 28′
1825·32	97° 46′	97° 43′	+ 3′
1833·27	61° 16′	61° 11′	+ 5′
1839 36	215° 51′	216° 2′	— 11′
1848·37	180° 6′	180° 6′	0

A better agreement could scarcely be desired. The observations made about the time of perihelion passage are liable to great errors in consequence of the excessive closeness of the stars, and therefore I did not take them into account in forming the equations of condition.

Sir John Herschel was obliged to admit large differences between these observations and the results of his theory, and these differences are considerably increased by using my elements. I am inclined to think that these observations cannot be satisfied without materially increasing the errors on both sides of the perihelion passage.

My elements agree very well with the latest observations which have come to my knowledge, as is shown by the following comparison:—

Observer	Epoch.	Observed Position.	Calculated Position.	Difference.
Lord Wrottesley .	1851·172	175° 55′	175° 52′	+ 3′
Mr. Dawes . .	1851·217	176° 35′	175° 49′	+ 46′
Mr. Fletcher .	1851·401	175° 58′	175° 34′	+ 24′

HERE I certainly considered that I had made a clue-up with this remarkable star; but circumstances having brought me to dwell within half a mile of Hartwell in 1851, an opportunity offered of continuing my observations on that and other objects, with less inconvenience than I had been at since leaving Bedford: especially as Dr. Lee had a *via nova* made between the grounds of Hartwell and St. John's Lodge, expressly for my convenience. I therefore resolved to watch γ at least till the final period of my predictions of her march (*see ante*, page 355); particularly as I had reason to suppose that the epoch of 1857 would exhibit the excentricity, inclination, periastral passage, and period, in greater unison than heretofore. Now my own angular measures for that—to me—crucial point, were a degree and a half in excess; while the places observed for me by my kind correspondents were—

POSITION	173°·00	DISTANCE	3″.640	EPOCH	1856·96.—*Airy.*
———	170°·08	———	3″·586	———	1857·35.—*Dawes (double image.)*
———	169°·93	———	3″·561	———	1857·42.—*Dawes (wire micr.)*
———	171°·55	———	3″·537	———	1857·41.—*Fletcher.*
———	171°·36	———	3″·665	———	1856·97.—*Lord Wrottesley.*

To these I must add another set from Captain W. S. Jacob, that was conveyed to me in a letter from Madras, under date of February 12th, 1858. This document was sunk in the Ava, a steamer laden with treasure, which was wrecked going into the harbour of Trincomalee; and, after its being almost pulpified by soaking in sea-water, was fished up from the wreck, and reached me on the 12th of May in that year. The following is an extract:—

I have the pleasure to acknowledge the receipt of your letter of the 2nd of September last, with its inclosures, literary and scientific * * * * In return, I send you my last epoch of γ Virginis—a mean of five nights' observations with powers 277 and 365—which, you will see, agrees very closely with yours:

POSITION 170°·69 DISTANCE 3″·504 EPOCH 1857·957

The vagaries of this pair (real or supposed) will, I suspect, turn out of the same character as those of 70 Ophiuchi (*see ante*, page 282) and α Centauri, which I have little doubt proceed from the disturbance of some planetary body or bodies, probably much larger than any of our planets. I have lately found out several other pairs exhibiting similar recurring disturbances; so that they will

form a class of their own, and be no longer exceptionable cases. The class will also include, of course, such cases as that of Sirius, with its recurring variations of proper motion. I hope to discuss the matter more fully before long.

It may assist examination, and save the trouble of raking for data throughout this rambling story, if we here recapitulate the whole series of my measures of γ Virginis; begging the reader to recollect that they are all resulting from the same eye, object-glass, micrometer, and mostly from the same eye-piece—so that for all observable movement they are fairly comparable *inter se*. On this account, only the operations with the large equatoreal are enrolled: but my personal acquaintance with γ commenced at an earlier period, and in 1828 I attacked her with some rock-crystal prisms charged in a 5-foot refractor of 3¾ inches aperture (Cycle, I. pages 385-6), and obtained with lens 2, prism 2, power 135·5—as the dependent results, by three sets of measures taken in the *nf* quadrant,

POSITION 88°·37′ DISTANCE 2″·72 EPOCH 1828·99.

The instrumental consideration thus given, applies indeed to all my printed observations; for, though I was charmed with the performance of the ocular crystal micrometer, I surrendered it for what I thought more in keeping with the objects in view (*see ante*, pages 125-6). Having made this statement, we will now proceed to a general résumé of the whole of my operations on this object, with the double-wire micrometer:—

POSITION	77°·9	(*w* 6)	DISTANCE	1″·6	(*w* 2)	EPOCH	1831·38
———	71°·4	(*w* 5)	———	1″·2	(*w* 3)	———	1832·40
———	63°·6	(*w* 7)	———	*not taken*		———	1833·23
———	62°·7	(*w* 8)	———	1″·3	(*w* 2)	———	1833·44
———	48°·8	(*w* 6)	———	1″·0	(*w* 2)	———	1834·20
———	45°·5	(*w* 5)	———	0″·8	(*w* 2)	———	1834·39
———	15°·0	(*w* 5)	———	0″·5	(*w* 1)	———	1835·40
———	*round*	(*w* 9)	———	*round*	(*w* 9)	———	1836·06
———	*round*	(*w* 9)	———	*round*	(*w* 9)	———	1836·15
———	*blotty*	(*w* 8)	———	*blotty*	(*w* 5)	———	1836·25
———	350·9	(*w* 4)	———	*elongated*	(*w* 5)	———	1836·30
———	348°·6	(*w* 4)	———	*elongated*	(*w* 5)	———	1836·39
———	265°.4	(*w* 3)	———	0″·6	(*w* 1)	———	1837·21

Position		Distance			Epoch
Position	235°·7 (w 3)	Distance	0″·8	(w 2)	Epoch 1838·28
———	217°·2 (w 5)	———	1″·0	(w 2)	——— 1839·40
———	192°·8 (w 5)	———	1″·9	(w 3)	——— 1843·08
———	191°·6 (w 8)	———	1″·9	(w 5)	——— 1843·33
———	185°·4 (w 6)	———	2″·1	(w 4)	——— 1845·34
———	182°·9 (w 7)	———	2″·6	(w 4)	——— 1846·39
———	181°·9 (w 6)	———	2″·6	(w 5)	——— 1847·41
———	179°·5 (w 8)	———	2″·8	(w 5)	——— 1848·36
———	178°·6 (w 8)	———	2″·9	(w 6)	——— 1849·31
———	177°·0 (w 7)	———	2″·9	(w 5)	——— 1850·28
———	175°·5 (w 5)	———	3″·2	(w 4)	——— 1852·42
———	173°·9 (w 5)	———	3″·2	(w 6)	——— 1853·35
———	171°·6 (w 6)	———	3″·4	(w 7)	——— 1855·40
———	170°·6 (w 6)	———	3″·5	(w 5)	——— 1857·41
———	169°·9 (w 7)	———	3″·8	(w 7)	——— 1858·39

These last places are, I presume, as fair as could be expected, the observations having been made under extremely favourable circumstances—as well instrumental as atmospheric; yet, from the object's present easy measurement, there is more discordance between the angular results of my correspondents and myself for 1858 than I expected, or quite like (*see ante*, page 247). γ however must be closely watched; for the motion is now so nearly uniform along the line of ellipse, and the variations both in position-angle and distance so slow, that multiplied observations become necessary, in order to eliminate as much as possible any errors of judgment.

While writing this, my youngest son, Captain Henry Augustus Smyth, of the Royal Artillery—arrives from Shoeburyness, where he has been experimenting with the new Armstrong gun, which strikes targets at two miles and sends shot to the distance of five miles. Turning now to a very different branch of applied mathematics, he volunteers to gather the *disjecta membra* of γ Virginis—of which star he has heard from his earliest years—and try his hand at moulding them into an orbit. I have therefore supplied him with the materials; and, if he can accomplish the task before his short leave of absence expires, his conclusions shall form a section of the Appendix to this volume.

After making the epoch of 1858, I took a real and formal leave of this object

(*see* No. V.), not from any relaxed zeal in its cause, but that at the age of 71 the morning and evening's going to and fro from house to house—from St. John's Lodge to Hartwell, and again from Hartwell to St. John's Lodge—began to be rather monotonous; and moreover it was high time to give place to young blood. To be sure some of my friends intimated that this was not a final resolution; and Sir John Herschel even made a prediction in this form—"Bid adieu to the observatory, eh? Not a bit of it. Years hence you will be found one winter's morning frozen at the end of a telescope, with your thumb on a micrometer screw." Now the chance for verifying this would rest on the equatoreal-instrument's being transported to my southern lawn, where certainly it would find but little rest, for happily I am one of those who hold that eyes are none the worse for being used. At all events, I have had the satisfaction of contributing to unravel a system which affords, by actual changes in angle and distance—the former varying in velocity inversely as the square of the distance—incontrovertible evidence of the physical connection of its constituent members. Every successive stage in this interesting inquiry has been converting probability into fact, and there cannot exist a reasonable doubt of this and other binaries being subject to the same dynamical forces which govern our own system. Yet there are some very formidable heterodox savans in opposition, who think that the operation of the law of gravitation among double stars, however probable, is not yet sufficiently capable of demonstration; for as the observations are only equivalent to seven or eight really distinct data, that those data are not sufficient to determine whether an ellipse is described according to that law. Now there are anomalies and bold hypotheses in the action of gravity; but as we know that binary stars are held together by a central force, and that the components revolve under the influence of a mutual attracting power, we can only postpone the question for time and increased accuracy of observation.

As this opinion, however, is not yet accepted; the various doubters must be allowed to swing in the tide's way, especially those whose opinion is worth having. Clairaut doubted Newton: placing too little faith in a doctrine which had otherwise proved its correctness and generality, this indefatigable geometer was led to

believe that the law of gravitation was neither true nor universal; but on trying back he was convinced of his mistake, and, like a true man, very becomingly chaunted a public palinody. Now, though there are certain impediments yet to overcome—and perhaps even *retard acceleration* difficulties to grapple with—every advance tends to demonstrate that the Newtonian doctrine of attraction, obeying the Keplerian law of areas, may be regarded as the controlling agent of the UNIVERSE: a most sublime and useful truth,—one chiefly added by a corps of zealous amateurs to the wonders of Astronomy!

ST. JOHN'S LODGE

CHAPTER IX.

OF ENCKE'S COMET.

Where is thy track throughout the vast expanse?
Still onward hast thou urg'd thy bold career,
From the first hour when the Creator's hand
Impell'd thy fire along the fields of light,
Nor ever yet arriv'd within the verge
Of mortal ken!

WEBB.

A detailed account of this gaseous wanderer will be found in the first volume of my Cycle of Celestial Objects; but, as a plate of its appearance on the 22nd of September, 1848, when caught up by my son, Professor C. Piazzi Smyth, then with me at the Hartwell telescope, has been prepared by him for this volume, a few discursive remarks may be added. When found, the comet was so faint as to require a large telescope and a practised eye to see it; and even then it appeared only as a dull nebulous patch about three minutes in diameter, a little condensed towards the centre, and shaded off indefinitely at the edges. It was best seen with the eye-piece powers of 50 and 22 times, and tolerable places were obtained by an eye-piece armed with a broad bar instead of wires, so as to render the gazer independent of an illuminated field of view. To my eye it seemed rather familiar, from having spent much time over it at Bedford, at its apparition early in November 1828; but the star which was accidentally shining through its central body, gave it somewhat the appearance of its having been furnished with a nucleus. (*See the Plate, and page* 377.)

At the very name of comet what a host of associations and ideas arise; what a mass of human errors and perversities does the history of cometology exhibit; what fear and dread have those wanderers so needlessly inspired! Never did popular prejudice adopt more unlikely engines for producing the threatened destruction of the world: without light or heat of their own, without any solidity, and without appreciable weight, how utterly powerless they must be for good or for evil!*

We now understand them to be as orderly members of the solar system as the planets: like them, and yet different from them; bearing the same relation to them that birds do to animals, both denizens of our earth, but the former able from their lighter nature to rise and perform flights in a medium into which the latter may never elevate themselves. So the comets—circulating like the planets round the self-same luminary, yet not possessed of their grosser weight—take courses and curious flights above and below the ecliptic, at all angles and in all directions, and with velocities and excentricities that seem for ever to be denied to the others.

Some have attempted to make out a connexion between planets and comets: and there were even hopes, before the discovery of Uranus and Neptune on the one hand, and of small comets of short period on the other, that on the outer confines of the system the excentricities of planets there to be found might approximate and mingle with those of comets. Recent discoveries have, however, shown the outermost planets to have the least excentricity, and the outermost comets the most; each of these respective species thus showing no tendency whatever to any amalgamation. But it is far more in the physical features of the comets, than in the character of their orbits, that they differ from planets; and the motions of both are understood, while the former's qualities are still as imperfectly known as ever they were in those days when they spread alarm and dismay amongst nations.

* That crazy and cruel fool—Alphonsus VI. of Portugal—being told that the Comet of 1664, described by Lubienietski, presaged the death of a sovereign, rushed out on his terrace, called it all sorts of bad names, and shot at it with his pistol! The comet steadily kept on its course.

That so little advance has been made in this department seems to be due chiefly, among those causes which we can qualify, to too much having been attempted; to inquiries having been made into matters not strictly within the reach of science. Thus, it was not enough to make statistical registers of the various genera of comets, and to investigate the routine of changes which each undergoes in the various points of its orbit, but men must needs account for how a comet was created, and what purpose it was subserving. In the dark ages these purposes were to foreshadow the deaths of kings, to indicate revolutions and wars, and—when those ideas were dissipated before an advancing philosophy—frost, storms, heats, droughts, &c. took their place. These, again, have vanished before the onward march of science; but, such is the perverse vitality of error, that men still think themselves bound to supply some cause and some purpose for the existence of comets. Against this, in the excellent work recently published under the title of Outlines of Astronomy, § 554, the illustrious author says,—"Even now that we have ceased to regard their movements as irregular, or as governed by other laws than those which retain the planets in their orbits, their intimate nature, and the offices they perform in the economy of our system, are as much unknown as ever. No distinct and satisfactory account has yet been rendered of those immensely voluminous appendages which they bear about with them, and which are known by the name of their tails (though improperly, since they often precede them in their motions), any more than of several other singularities which they present."

Here now there seem to be far more stringent requirements than are exacted in the case of the planets; for who has ever thought it a drawback on our knowledge of planetary astronomy, that we did not know the office which these bodies perform in the economy of the system? Enough for us, then, that we can determine with greater exactness every year, their distance, size, weight, the form of their surface, with the motions and characteristics of their atmospheres; and the sooner we apply ourselves to steadily accumulating similar data respecting comets the better; for certain glimpses of the wished-for desiderata may then appear of themselves. Royal roads and short cuts have, however,

seductive charms for the despisers of induction; and I will here give some passages of a letter lately written (August, 1850), by an assistant-Astronomer in a standard observatory, as a sample of side-wind philosophy:—

Je m'occupe avec les sciences physiques, et j'ai fait quelques découvertes très intéressantes, dont vous verrez bientôt l'une qui exprime la cause du magnétisme terrestre.—J'ai trouvé aussi un système pour supprimer les incendies. Ce système a été déja approuvé par le Comité "select" de Woolwich, et à present je m'occupe à le mettre en exécution.—J'ai trouvé que la terre et les planètes subirent plusieurs périodes géologiques, et pendant chaque période des ages sont parcourus; qui se distinguent par leur températures. Le nombre des périodes parcourues par chaque planète est différent, quoique elles ont été produites par le soleil à la même époque. Maintenant, les planètes parcourent des ages différentes. Saturne parcoure son age glaciale, Jupiter son age tempéré, et Mars son age torride, la Terre se trouve dans son age temperé. Ces ages dépendent de l'épaisseur de l'atmosphère, qui se forme par la décomposition de l'eau. L'azote n'est pas un corps simple, mais un composé de 2 hydrogène et $1\frac{1}{2}$ oxygène. 4 atomes d'eau$=4\ O + 4\ H = O + 2\ (1\frac{1}{2}\ O + 2H) = O + 2\ A\ 2 =$ air. Ainsi les mêmes elements produisent l'eau et l'air.

Les comètes sont des atmosphères détachées en deux moitiés par les poles des planètes à la fin de chaque période géologique, ou au moment de cette catastrophe par la séparation de l'atmosphère, les corps organisés cessent de vivre, la température baisse, et les eaux accumulées dans la zone torride par la pression de l'atmosphère dans les latitudes supérieures, où son épaisseur était la plus grande, se déchainèrent, et elles formerent des torrents dirigés vers les poles, qui apportèrent les débris des terreins pour ensevelir les cadavres dont la surface de la terre était jonchée. On a été étonné ici dans les musées quand on a entendu qu'il n'existe pas des fossiles d'animaux terrestres dans la zone torride. Les élémens des comètes sont les mêmes que ceux de l'atmosphère, 1° de l'eau, 2° des vapeurs, et 3° de l'air. Ainsi les comètes ne sont que des vents de l'espace.

Here is a precious theory, built up without a single fact, and at variance with all experience! First of all, azote, suspected to be a compound gas, but never yet proved to be so, has its composition exactly laid down from fancy, so as to agree with a supposed easy plan for nature to manufacture the gas on a large scale. Then we come by a jump to the comets, which are portions of the atmosphere detached from each pole of the earth, at stated periods. As to how this detachment is brought about, why it should be, and when, not a glimpse is given; nor of how the detached air is at once to take upon itself the peculiar motions and characteristics of a comet, so excessively different to all that the body of air had been accustomed to, when still accompanying the earth. All these difficulties are, however, overlooked, and the

notable conclusion is at length come to, that the elements of comets are the same as those of the atmosphere, and that thus the once-thought portentous comets are but the winds of space!

If, instead of such constituent principles as these, some additional good honest elements in the shape of perihelion distance, excentricity of orbit, &c. of comets had been given, some real approach to a knowledge of the physical characteristics of these bodies might have been obtained. The intelligent and elaborate discussions of Professor Encke on the phenomena of the very comet before us, constitute a point which might have been imitated, however hopeless the *excelsior* might prove. An inquiry into this philosopher's *Constant of Resistance*, so as to tend to a definitive solution of a question which gives such inquietude to astronomers, might have obtained preference over a mere bag-full of wind: and though the scrupulous devotee may still hold the point of a medium to be in abeyance, there are those who consider the undulatory theory to be incontestably established. Nor can the general argument as to whether our comet shall, after a great but not infinite period of time, be absorbed by the sun or vaporized into attenuated ether, be a matter of indifference even to the above-quoted air-man.

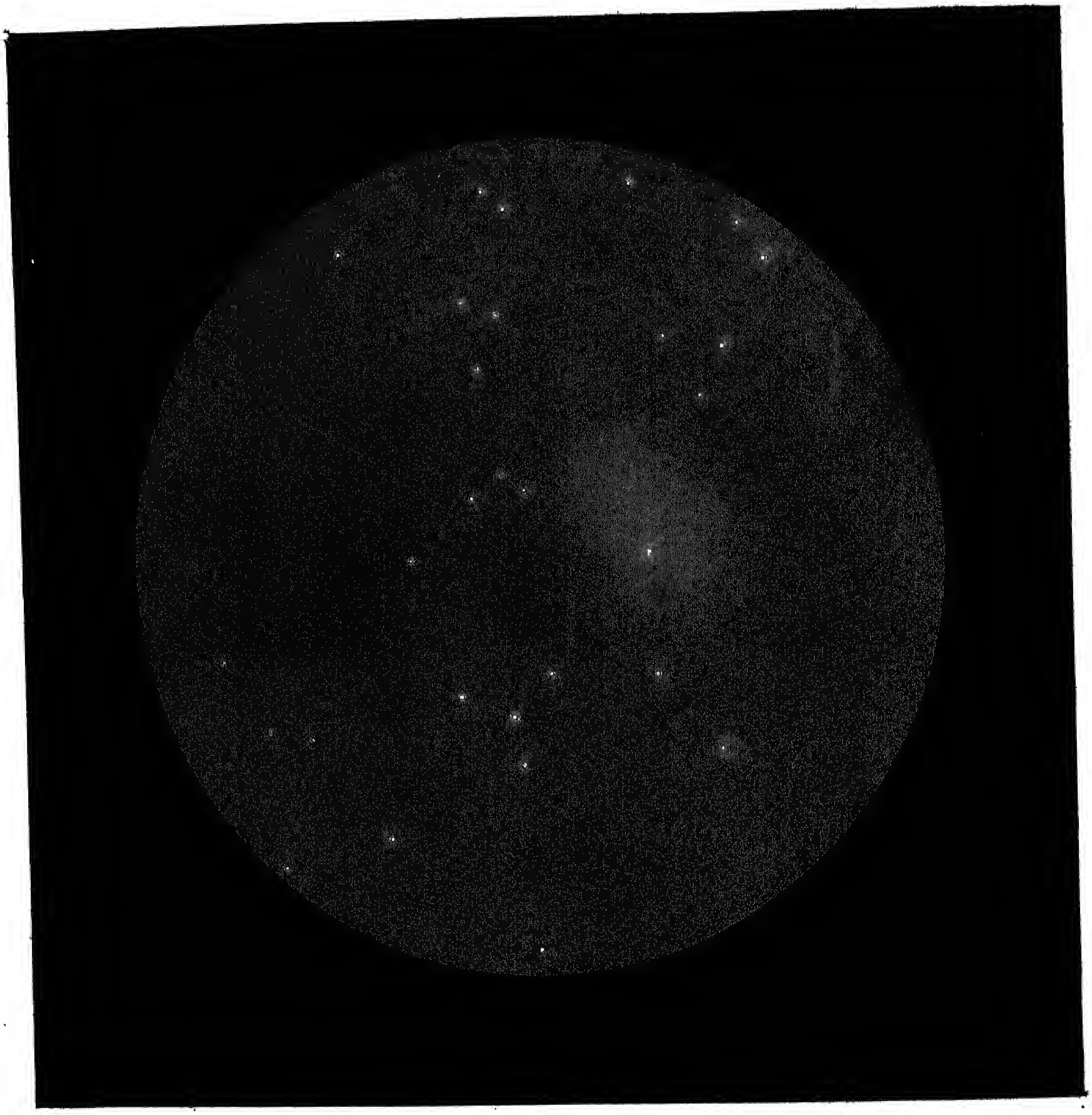
Investigations into the orbits of comets can fortunately be pursued without any knowledge of their form and size, by mere reference to the point called the nucleus; all the sensible weight of the body is so completely centered here, that, however much further the body may extend on one side than on the other, it makes no difference,—no sensible difference at least,—in the position of the centre of gravity and of motion. With the planets, we may either observe the centre, or the two limbs, and the mean of these will be seen to give the centre, but it is far otherwise with the comets: at the tail side the body may extend sixty to one hundred degrees from the nucleus, at the other side only one-fourth of a degree; but yet the nucleus moves as if wholly independent of all that excentric body. This body, both the tail part and the denser parts about the nucleus, called variously the coma or the head, and often mistaken for the nucleus itself, are constantly altering in size and form; the nucleus is the only portion which remains constant; it is infinitely small in size,

is seen only as a minute stellar point, and to this alone the attribute of solid matter can attach. To this nucleus proper (not the head, as noticed by old observers) no sensible magnitude has ever been attached, and every succeeding improvement in telescopes further limits the size which it can possibly be of. The smaller comets do not show any apparent nucleus; the existence of one, however, though too small to be seen even in our best telescopes, may always be inferred from the tendency of the gaseous matter to concentrate round one part, and so form the head, or at all events to indicate the referring point for observation of the place and orbit of the comet.

Both the nucleus and the gaseous semi-transparent body shine by reflected light, as proved by Arago's polarising experiment, and cannot therefore be supposed to have any notable temperature of their own; certainly not any heat of incandescence. The small weight that the nucleus can possibly be of is indicated sufficiently in mere observation by its excessively small dimensions; and the little weight of all the body is shown in its transparency; for it can be seen through in every direction, and so easily, that a thickness of sixty thousand miles of it (according to Sir John Herschel in the case of Biela's comet) does not sensibly affect the brightness of the smallest stars. Hence there was probably not so much substance and matter in all that depth and thickness of comet as there would be in a depth of a few feet, or even inches, in an ordinary cloud.

Comets were anciently divided into twelve classes, in this fashion—bearded, torch-like, sword-like, tun-like, javelin-like, horn-like, &c. &c.—and even still a distinction is kept up between the large comets and the small ones, or those with tails and those with none: but this would seem an unnecessary division, for the one sort merge so imperceptibly into the other, that the features of each may be represented merely by different degrees of the same form or quality. In times when they were less feared, and perhaps better understood, these "blazing-stars" were subdivided into bearded, tailed, and hairy or comatose: and for more refer to the Cycle, (I. pages 210, &c.)

Comets of long period, as that of 1811, with a period of two thousand



HALLEY'S COMET.

As seen at its re appearance on the [illegible] of September [illegible]

At the Hartwell Observatory.

five hundred years, are generally large; the nucleus of this was distinct, the tail long, broad, and bright; and, being circumpolar, remained constantly visible for upwards of ten months (*see ante*, pages 91-2.) And comets of very excentric orbits, as in the instance of 1843, which had hardly any perihelion distance, have very excentric bodies, or long narrow tails, this appendage being at one period 70° long by 1° broad.

Comets of short period and small excentricity, as Encke's, with a period only one thousandth of that of 1811, have small bodies, and these not very excentrically dispersed about the nucleus. Thus Encke's, as exhibited in the engraving, presents only a small oval mass of vapour, without any visible nucleus, but with a sensible concentration towards the place of it. A small star is shown, as it it was seen, clearly and brightly, through the very thick of the comet, and must not be confounded with the nucleus. The rest of the stars are inserted rather to show the telescopic characters of them, and their distinction from the comet; they so sharp, precise, and pungent,—and it so weak, diffuse, and ill defined. While planetary bodies may be fixed in space by means of their boundaries, may, in fact, be defined in the strict geometrical sense of a definition, the comet admits of no such fixation, but recourse must be had to the new natural-history method of the type; which, instead of drawing the circumference of a circle, and stating all that it excludes, rather fixes the place of the centre of it, and gives all that it eminently includes: so the place of the comet cannot be defined by its borders, but by the position of its centre, or we may say the excentric focus, carrying the nucleus and the more condensed matter immediately around it.

Although I have referred the reader to the account which I have given in my Cycle of Encke's comet, still the interest attached to its discovery, or rather to the determination of the nature of its orbit, and consequent rate of its motion, warrants a Parthian glance. As Encke's investigations produced the surprisingly short period of one thousand two hundred and eight days, a retrospective view was obtained, and by trying back it was found to have been seen in 1786 by Messier—*Le Furet des Comètes*, by Miss Herschel in 1795,

and by two or three observers in 1805: but it was so small and so difficult to be seen even with telescopic aid, and the duration of its appearance was so brief, that the only wonder is that it did not remain unnoticed in the heavens.

The whole astronomical world hailed with the greatest delight the discovery of a comet of short period, one so very short as to keep the body constantly within, and far within, our planetary region, thus bringing it frequently to our view, instead of driving it to such distant regions beyond, as to lead some men to think that the other focus of the orbit must be formed by some star, or remote sun. But, though it might be interesting, we can neither lug in the argument, nor the question as to the rare elastic ethereal medium diffused through universal space (which its frequently observed returns somewhat countenance, to the great joy of the undulatory theorists), on the shoulders merely of a drawing of the physical appearance of the now well-known vagrant.

But, referring to the inference given in the Cycle, (I. pages 251-3,) however anomalous it may seem to the motions of the bodies of the solar system, the alarming circumstance has been further disclosed, and become an all but indisputable fact, that Encke's *periodic time* actually does undergo a slow, gradual, and apparently regular decrease, indicating a consequence of the gravest physical importance. After the known perturbations, and all other disturbing causes, have been discussed and equated, the effect of resistance is clearly perceptible; and, though that effect has been considerably eliminated and reduced by the skilful labours of geometers, it is rendered thereby more conspicuous. In a word, it may be stated that the comet's period between 1786 and 1795 was 1208½ days, but in 1852 it was found to be only 1204 days; so that its path must be a sort of elliptical spiral line continually winding inwards to the centre, the successive coils of which are very close together, every successive revolution bringing the body nearer and nearer to the sun—thus diminishing in velocity and revolving in less time. Now it is to be apprehended that a resisting medium, such as the luminiferous ether of the undulatory theorists, being converted from a hypothetical fluid into the real *vera causa* of physical agency, together with the opposition suffered from the denser ether around

the sun, indicate that the system is not made to be eternal. To be sure philosophers are still at fault, and one of the stoic school asks, "What need we care about that?" But it is impossible for a contemplative mind to revert to the positive effect of such definite resistance without solicitude and awe; and we may venture to predict it will be well pondered over before 500 years shall have passed away. As the topic is still a tender one in certain quarters, we will wait a little.

The same delicacy, however, and wish to confine ourselves to our own subject, will not prevent an allusion to the contraction observed in the size of the body of the comet on approaching the sun: for with this is intimately bound up all and every change and characteristic in the wanderers. Hevelius had first of all noticed that comets (large ones, of course, visible to the naked eye) contrasted in size on approach to the sun, and *vice versâ;* but the observation was strangely overlooked, and precisely the contrary opinion gained ground. At length, a few years ago, M. Valz pointed out that Encke's comet regularly contracted in diminishing its perihelion distance; this contraction he attributed to the pressure of the dense ether in the neighbourhood of the sun. This cause Sir John Herschel showed to be insufficient, and he supplied a better, but here the matter stopped; and, so far from applying the same theory to large comets, the contrary law was still supposed to obtain with them; they were considered to "throw" or "shoot" out their tails in approaching the sun; and to retract them, if not to lose them altogether, on leaving him; thereby proving them to be an entirely different genus to the small and tail-less comets.

More exact observation, however, aided by the fortunate appearance of so extreme a comet as that of 1843, and a calmer consideration of the facts of the case, independently of any previous theoretical ideas, has shown that all comets decrease in size as they come to perihelion: that the length of the comet varies in a certain proportion with the radius vector; that, consequently, in the case of a large one like that of 1843, with hardly any perihelion distance, it becomes at that part of its orbit, where it remains but for a moment,

so very dense, as to be capable of being seen in the day-time, yet expands so rapidly after that, as to be soon lost sight of again even at night. In the case, however, of a comet like Encke's, small in itself and with little excentricity, it never experiences much concentration of substance, and therefore never becomes particularly bright: and here, with due deference, I must lodge a dissent from a friend's statement respecting its being of a tawny tint, or fawn colour; for the effects of gaslight on the observer's retina, and those of atmospheric media on the object, were assuredly in the instance here alluded to—and stoutly insisted upon—not duly considered.

That comets' tails should have been supposed to be produced at perihelion, seems only to be attributed to their becoming then more visible than at other times; they are then nearer to us, and hence seen under a larger angle; they are closer to the sun, and consequently illuminated with a stronger light; and thirdly, they are then more dense, and therefore capable of reflecting greater light. Partly, perhaps, also owing to the quickness of the comet's motion at perihelion, by which it is transferred in twenty-four hours from the daylight part of the sky to the night: and thus a tail before existing but not seen by reason of the light back-ground, is said, when seen brightly on the night-sky, to have been suddenly shot out there; for, be it remembered, no one ever pretends to have seen it shooting forth.

Again, as to the phenomena of the form of a tail, as far as such an ill-defined shape is concerned, this is pretty clearly proved by the comet of 1843 to be an affair of phase; and there is strong reason to believe that every comet is a complete elliptical figure, with the nucleus in the focus nearest to the sun. The phase becomes strongest, or the tail-appearance most manifest, in large and dense comets, and least in small and diffuse ones, or in those which may be concentrated within a very small space when their perihelion distance is very small. But this is not exactly the place to explain the whole range of phenomena; it must suffice to point to our figure of Encke's comet. Its smallness, together with its diffuseness and little excentricity, causes the illuminating rays of the sun to strike upon it, and be reflected back to us with almost equal

intensity from every part (independently, of course, of any inherent and proper form in each portion to reflect light as from greater or less density): here, therefore, we see the whole body of the comet; namely, an oval vapourous mass equally diffuse on every side, and most condensed about one focus, which is ever that nearest to the sun.

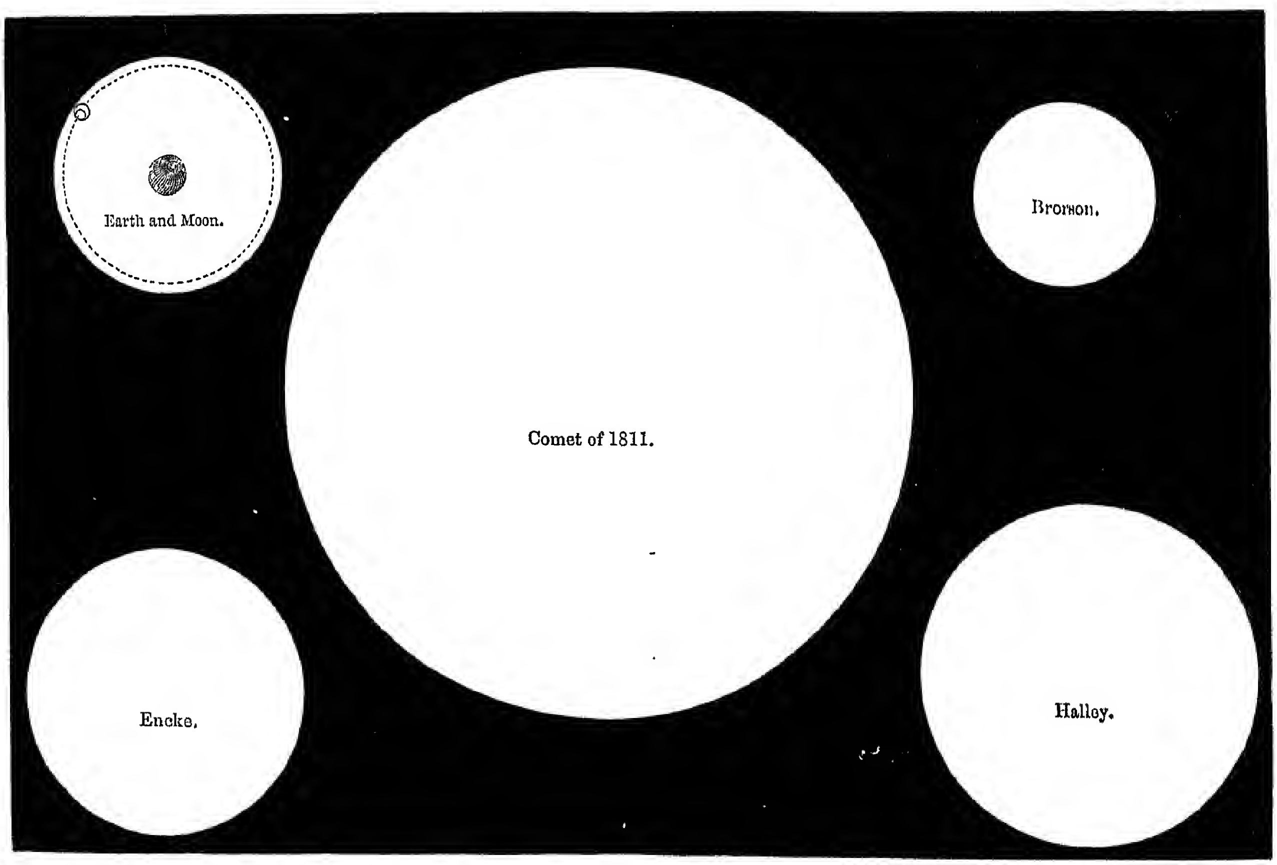
We will now give the elements of our comet, as well those which M. Encke obtained from a masterly discussion of the earlier observations, as those of the verified returns:—

Date of Perihelion Passage.	Longitude of Perihelion.	Ascending Node.	Inclination.	Perihelion Distance.	Excentricity.
	° ′	° ′	° ′		
1786, January 30·9	156 38	334 8	13 36	0·334	0·848
1795, December 21·4	156 41	334 39	13 42	0·334	0·848
1805, November 21·5	156 47	334 20	13 33	0·340	0·846
1819, January 27·1	156 59	334 33	13 37	0·335	0·848
1822, May 23·9	157 12	334 25	13 20	0·346	0·844
1825, September 16·2	157 14	334 27	13 21	0·345	0·845
1829, January 9·3	157 18	334 30	13 21	0·345	0·844
1832, May 3·9	157 21	334 32	13 22	0·343	0·845
1835, August 26·2	157 23	334 35	13 21	0·344	0·845
1838, December 19·0	157 27	334 37	13 21	0·344	0·845
1842, April 12·0	157 29	334 39	13 20	0·345	0·844
1845, August 9·4	157 44	334 20	13 8	0·338	0·847
1848, November 26·1	157 47	334 22	13 9	0·337	0·848
1852, March 14·4	157 51	334 23	13 8	0·337	0·847
1855, July 1·3	157 53	334 26	13 8	0·345	0·847
1858, October 18·5	157 59	334 28	13 4	0·346	0·846

From which it is seen that the motion is direct, with an aphelion distance of about 390,000,000 English miles, a perihelion distance of upwards of 32,000,000 miles, and a period of revolution amounting to only 3·296 years. Friend Arago, with a bias not at all unusual to him, has laboured to assign its discovery to Pons, and the finding of its periodicity to Bouvard's parabolic elements; but without impinging on the high merits of both the Observer and the Computer, we may remind the reader that no parabolic orbit could represent the observations within the limit of error, and it is incontestable that the rigorous and laborious investigation of Encke led to the first detection of a comet of short period—and that it had been noted previously to 1818.

The representation on the copper-plate attempts to give the comet just as seen in the telescope, with the view of rendering every one who has the drawing in

possession, to be as well circumstanced for judging of the phenomena as those who saw the comet on the night in question. But, nevertheless, the representation falls much too short of what an astronomical drawing should be, of which the necessary accompaniments ought to be—statements of the probable error of the magnitude and brightness of each part. This, however, cannot be attempted in the present backward state of this branch of astronomy; but the more backward it is, and the more it is neglected by others, the more room for some one to distinguish himself, and perform real good work, in adding to the general stock of astronomical knowledge. Cometography being necessarily joined with the determination of the magnitudes and colours of the stars, I cannot but again recommend both it and them: and we will close this chapter with the comparative diameters of the heads of four remarkable comets, having the earth and the moon's orbit around it, introduced as a scale—



CHAPTER X.

THE METEOROLOGICAL DEPARTMENT.

Pour regler nos travaux, pour marquer les saisons,
L'art divisa du ciel les vastes regions.
SOLEIL, ame du monde! Océan de lumiere!
Douze astres différents partagent ta carrière.

DE LILLE.

If the reader who may have accompanied us thus far, will kindly refer to the evidence of solar influence alluded to in pages 25 to 32 of this work, he will perceive that we have arrived at the conclusion that all, or nearly all, the changes which now take place at the surface of our globe, are due, whatever modification may be assumed, to the emanations and actions of the sun. The proximity of the moon, too, renders her also a predominant cause; and as by their forces unequally exerted over its surface they raise the tides in the ocean, so they must likewise agitate the atmosphere with a corresponding reciprocation. Electricity, magnetism, light, galvanism, heat, and all their affinities, have doubtless a common source; and when that source shall be propounded by some master-mind—another Newton, and the alternating periods of fine and foul weather experienced in every part of the known world come to be ascertained, we may know something about Meteorology. Could we unravel the intricate maze, we might trace the true alternating agent of each distinct motive action affecting the atmospherical tides, and hence deduce the ultimate effect on the weather as arising and driven by the compound operation. Much of the regular or periodic

winds, and a large share of the aqueous circulation, obviously result from solar action, in combination with the earth's rotation on its axis; and many a minor consequence is easily accounted for by local peculiarities. Herein hypothesis and fancy offer warm enticements to speculation; but to acquire a proper knowledge of, and a mastery of the subject, still demands the strenuous and continued aid both of theory and practice, for, so to speak, we absolutely know but little as yet. Still the barrier to a better acquaintance with φυσις is assailable, since she seems to keep no secrets hidden from resolute inquiry: but the searcher after truth must never forget the Newtonian axiom—"No more causes are to be admitted for the explanation of any phenomenon, or class of phenomena, than are true and sufficient." Astronomy, chemistry, and geology have surmounted many formidable and apparently impracticable impediments; and who can give an opinion as to what meteorology may accomplish before a century hence—

> Nature well known, no prodigies remain,
> Comets are regular, and the tides are plain!

My late esteemed friend, John Frederick Daniell, who expired on duty in the Council-room of the Royal Society, observed that "Man may with propriety be said to be a meteorologist by nature;" because watching the atmospheric vicissitudes, on which he is so dependent not only for his comfort, but even for his subsistence, is a necessary portion of the labour to which he is born. Investigating the weather is but studying the conditions and variations of the air, as influenced by the elements of rest and motion, wind and calm, heat and cold, moisture and drought, together with other similar particulars: still the study is to the philosopher one of interest and delight—to the observer of nature it affords objects of grandeur and sublimity—to the farmer, the florist, the traveller, and the physician, it is in some measure a study of necessity—to the seaman it is yet more especially and urgently important—being a sworn auxiliary to navigation. But, though these phenomena—the vital stimuli of organized beings—have consequently occupied the attention of all classes of the community from the earliest ages, it is in comparatively recent times that the

inquiries of Redfield, Reid, Dove, Maury, Espy, and others, show that, capricious as the weather appears to be, it is nevertheless certain that it observes laws as stable and constant as those that govern all other natural phenomena.

The actual science of meteorology, however, has languished for want of the spirited and pointed aid which is given to other branches of physical inquiry by associated bodies of votaries. Many valuable registers were kept of which the mere numerical mean values were known, while others were entirely lost from want of being classified and reduced. It therefore struck a few amateurs that, in order to secure the advantages of arrangement, publication, and well-concerted combination, an express association ought to be formed; and accordingly, on the 4th of April, 1850, a meeting of some friends of the science was convened by Dr. Lee at Hartwell; where, in the library, they agreed upon a general system of observation, uniformity of registry, systematic communication, and other measures for insuring precision to the advancement of the aëro-statical branch of physics. At this decisive meeting, the gentlemen present elected Samuel Charles Whitbread, Esq. of Cardington, near Bedford, as their President, appointed an efficient Council to carry out regulations, and thus established the present British Meteorological Society. The aim and principal objects contemplated were thus enumerated:—

1. A collection of correct manuscript observations.
2. The publication of tables to be used in calculations.
3. The reduction of observations to useful results.
4. A collection of all observations of the same phenomena.
5. The formation of a repository to which observers may consign the results of their labours.
6. The distribution of meteorological papers.
7. The examination and correction of meteorological instruments.
8. The encouragement and promotion of meteorological science.

On many points of this paramount inquiry, it must be confessed that experience has anticipated theory; and intelligent diligence—without which no useful knowledge can be rendered effective—has still to detect the art which Nature uses in governing the physical forces and complicated alternations of Meteorology. Yet the general bearing of the local climate is pretty well

approximated by the weather-records which Dr. Lee has had regularly kept since 1829 to the present time: from which, and from the many observations I have been enabled to make, and direct to be made, in numerous visits during each of the elapsed years, some tolerable conclusions may be drawn. My remarks here, however, will principally relate to a group of ten years; for the highly improved scheme of register introduced lately by Mr. Glaisher, of the Greenwich Observatory, will no doubt form a future feature of the Hartwell climatology. From the data already collected, it is seen that the usual annual extremes of weather take place in January and July; but that certain conditions of wind, and other atmospheric phenomena, occasion interruptions, so that the greatest cold may occur in a range between December and March. In rapidly wading through the recorded columns, the prevalent winds appear to be those from W.S.W. to S.S.W., and the E.N.E. wind is the coldest in spring. The most intense heat happens during settled weather and south-easterly winds in the summer, the maximum of the thermometer occurring between 1 and 2 P.M., and the minimum just before sunrise. This may be further illustrated, by submitting the general annual mean carefully derived from the average monthly results of the register for 1837; a year in which I had occasion to pay a marked attention to the details, in order to compare them with a summary of those observed at Oxford and Bedford: and moreover it was a year remarkable for the establishment of simultaneous magnetic observations over the globe—to the which I was an earnest though humble accessory. In this attempt two barometers were usually noted, but the reliance was upon one supplied by Mr. Thomas Jones, of Charing Cross, for the observatory—the cistern of which is about 400 feet above the level of the sea; and the principal thermometer is the self-registering one of Mr. James Six, as described in the Philosophical Transactions for 1782. There were three hygrometers, but that selected for the comparison in hand was similar to that by which I obtained the dew-point at Bedford; it was made with twisted filaments of the *andropogon contortum*, a grass of extreme sensibility, brought from India by my friend the late Captain Henry Kater (Cycle I. page 345). The indications of

this were shown by an index graduated to one thousand parts, from immersion in unslacked lime to saturation in moisture; and these parts were afterwards reduced to degrees of a circle and tenths, whence the dew-point was approximately deduced. During 1837, there fell 22·81 inches of rain; and the predominant winds were S.W., S.E., N.E., and West, the least provalent being the South. This is the general summary through that year:—

	MAX. Inches.	MIN. Inches.	MED. Inches.
Barometer . . .	29·92	29·02	29·47
Extremes .	30·41	28·30	
Thermometer . .	60°·7	41°·8	51°·25
Extremes .	81·4	20·6	
Hygrometer . .	70·80	48·4	59·6
Dew-point .	55·6	37·0	

In the year 1839, the east and west winds, with currents from their subordinate rhumbs, were the most frequent that blew, as if alternately condensing and dissipating the weather-waves. The quantity of rain which fell was to the amount of 27·30 inches; a fall of one inch over an acre of land being nearly 23,000 gallons. The average pressure of the atmosphere for that year was 29·97; and the mean temperature was 49°·15—being 46°·13 for the spring, 61·11 for the summer, 50°·37 for autumn, and 39·01 for the winter; the several subdivisions being—

January . . .	38°·49	July	62°·65
February . .	38°·92	August . .	61°·49
March	40°·05	September	57°·74
April	47°·13	October . .	52°·16
May	51°·20	November .	41°·22
June	59°·18	December .	39°·63,

I may here state, although perhaps it is barely necessary, that by the *seasons* above given, the common division is meant; namely—under SPRING, the months of March, April, and May; under SUMMER, those of June, July, and August; the AUTUMN consists of September, October, and November; and the WINTER is during December, January, and February. The transitions of the seasons, the alternations of day and night, the changes of old and new

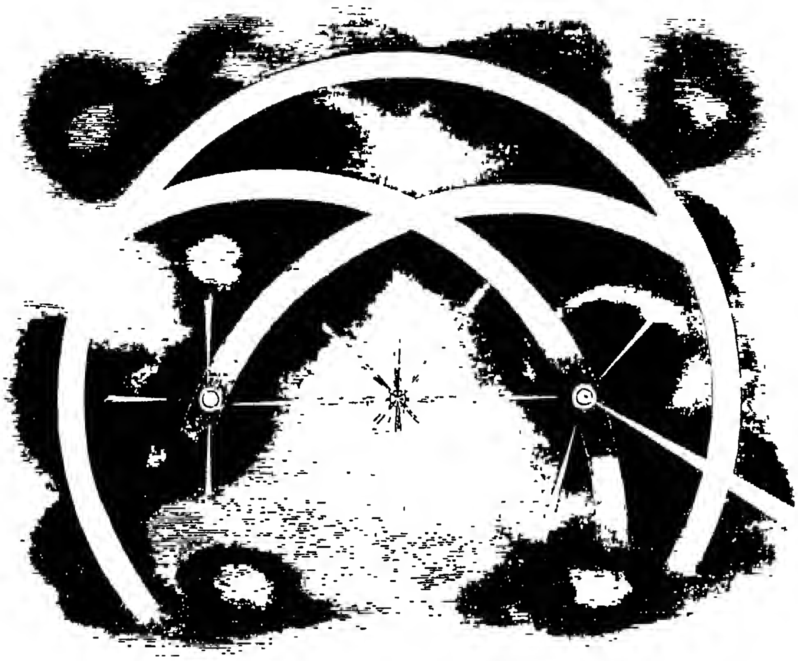
years, and other physical movements pertaining to the laws of nature, form a counterpart or image of the vicissitudes of human life. Hence the wind up of Thomson,—

> Behold, fond man!
> See here thy pictur'd life; pass some few years,
> Thy flowering Spring, thy Summer's ardent strength,
> Thy sober Autumn fading into age,
> And pale concluding Winter comes at last,
> And shuts the scene.

Still, where content exists, winter has its amenities. Thus Cowper—

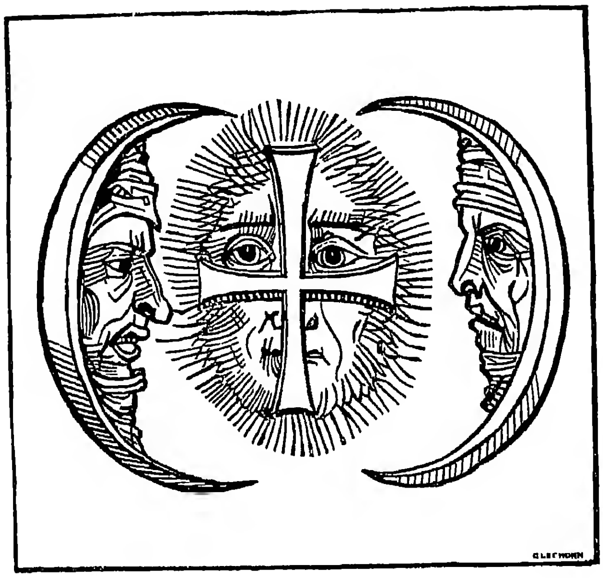
> Now stir the fire, and close the shutters fast,
> Let fall the curtains, wheel the sofa round,
> And while the bubbling and loud hissing urn
> Throws up a steamy column, and the cups
> That cheer but not inebriate, wait on each,
> So let us welcome *Winter's* ev'ning in.

Among the phenomena noted at Hartwell, I crave admission for one on account of its affording a plea for a preposterous vision of the mediæval sages, and thereby relieving them from the grave charge of prepense fraud. On the 23rd of March, 1853, a very remarkable lunar halo and paraselene were seen from 7^h 8^m P.M. to 8^h 16^m, attaining its maximum brilliance at 7^h 40^m, when the two mock moons were remarkably bright, and of various colours. The paraselene on the east of the moon was of a uniform whitish colour slightly tinged with orange—that on the west exhibited decided prismatic hues, being blueish towards the moon, and a reddish yellow on the opposite side—while the large halo was nearly circular, and its diameter measured about 52° from east to west, the semi-diameter being equal to the apparent distance between η and β Ursæ Majoris. At this time the evening was very fine, with light airs from the N.N.E. quarter—the barometer being 29·35 inches, and the thermometer 31°. The following accurate engraving of it is reduced from a large drawing made by the ingenious Mons. F. V. Fasel, who watched the commencement, progress, and dissipation of the pageant, from the vicarage at Stone—



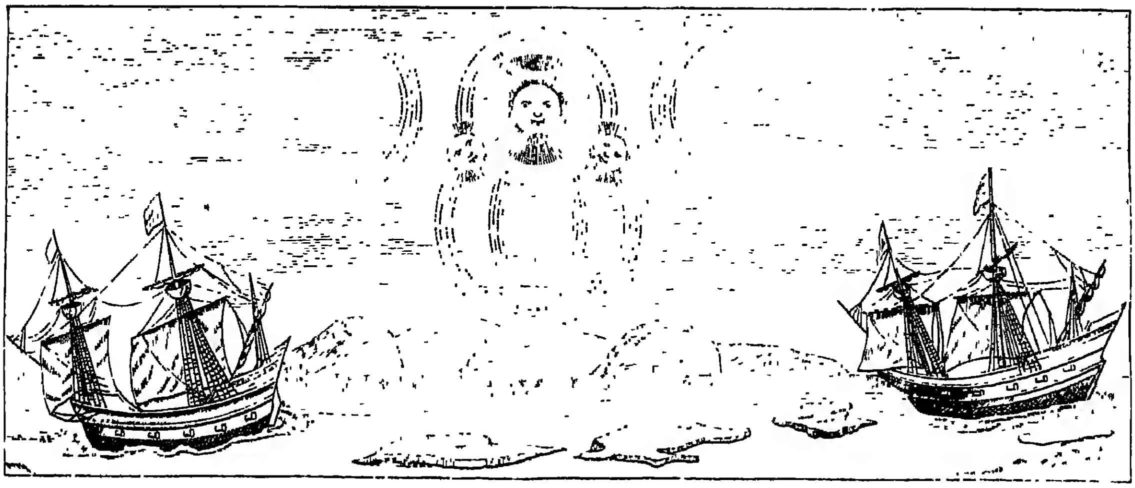
Sir John Herschel has remarked that very strange things were seen in the heavens before telescopes came into use; and that optical invention did not dissipate them, I have shown in the Cycle, (II. page 228,) where I have exhibited the *sudarium* of S. Veronica as seen by Padre De Rheita, with his binocular spy-glass. Now the sages of the Nuremberg Chronicle assure us (fol. CCIII. *b*), that in the year 1150 A.D., after the Emperor Frederick—*cognomento Barbarossa*—had carried fire and sword into Lombardy, "with the valour of a soldier and the cruelty of a tyrant," there were seen, for about two hours on the 9th of September, while the sun was in the west, three human faces on its disc before it set. In a similar way, in the beginning of the year, three moons appeared with a holy cross in the midst; which "visions were interpreted to indicate great discords among the Cardinals in electing a Pope." There were also many cities shaken and people killed by earthquakes during those unhappy schisms. This is a copy of the representation given in that

portly tome, and the good-natured reader will make allowance for the ignorance, superstition, and troubles of the time—



Human faces were no doubt seen aloft from Plutarch's days to those of Barbarossa and the contending Popes, when the above phenomenon appeared; and the pleasing hallucination lingered through the dark ages, to the no small profit of the astrological seers who preyed on the ignorant. More than four hundred years after Barbarossa had been gathered to his fathers, the same impression regarding parhelia obtained; and we can produce a still better representation of one than the Nuremberg sages could procure for their xylographer. In the third voyage by the Dutch to find Cathay and China, under the leadership, though not command, of that persevering and determined navigator Willem Barentsz, who was rated chief pilot (*opperste stuerman*) to Jacob von Heemskerck, a noble who was not a seaman, we find such an incident recorded. While the two ships forming the expedition were battling among the haunts of sea-horses, and "cruel big bears," they suddenly saw a wonderful sight in the sky, or rather sign in the heavens (*Een wonderlijck*

hemel-teijcken), which was reasonably enough described by De Veer, an eye-witness, whose narrative was translated by William Philip, at the *intreaty* of Richard Hackluyt, in 1609. The passage in question is thus rendered—"The fourth of June, 1596. And when the sunne was about south-south-east (½ p. 9, A.M.) wee saw a strange sight in the element: for on each side of the sunne there was another sunne, and two raine-bowes that past cleane through the three sunnes, and then two raine-bowes more, the one compassing round about the sunnes, and the other crosse through the great rundle (arc or circle of the larger rainbow); the great rundle standing with the uttermost point elevated above the horizon 28 degrees." This is sufficiently simple and clear; but it is evident that the whole of the hardy crew did not use the narrator's barnacles, for the following view of the spectacle is faithfully copied from the plate which illustrates the original publication: and moreover there is the anachronism of a day in introducing the lumps of ice which are floating about, for Gerrit De Veer expressly says—"The fifth of June wee sawe the first ice, which wee wondered at." The reason of the wonder was, that a man walking the forecastle suddenly and loudly sung out that swans were in sight—"which wee that were under hatches hearing, presently came up, and perceived that it was ice that came driving from the great heape, shewing like white swannes."



What has been above advanced, may be deemed to afford a tolerable view of the climature of the place for the present; but, as before remarked, the improved system of observation and registry of the weather introduced at Hartwell will inevitably yield results of a more accurate character. And the object is well worth pursuit; for, though certain branches of meteorology may long remain among the physical desiderata, yet much may be achieved respecting clouds, vapours, winds, thunder, lightning, hail, rain, ignes fatui, and other perceptible phenomena of the lower regions of the atmosphere, which cleanse the air—that means of health, invigorating the weak and refreshing the weary. Considerable advance has already been made in inquiries as to the luminosity, temperature, gravity, moisture, and all the come-at-able agents of our envelope; but those questions are so beset with difficulties, that our utmost knowledge in this respect does not grasp sufficient facts to reduce the various and uncertain phenomena to formal rule, or to establish anything like a certain theory. The zeal now exerted, however, in various observatories, as evinced by many published and widely circulated registers, will, very shortly, set the long-prevalent fancy for the lunar cycle at rest, a belief by which the revolutions of the moon's nodes are to place everything exactly as it was nineteen years before. Chemistry and geology have lately surmounted formidable impediments, and achieved wonders; and who dare pronounce an opinion as to what meteorology may become, even before the lapse of another century?

In the explanation of the earth-thermometer performances, given by my son C. P. Smyth, in the XIth volume of the Edinburgh Astronomical Observations, he delivers his conclusion respecting the amount and probable inconstancy of the heat given off by the sun, for which theory has no guarantee. Four long thermometers were carefully inserted at different depths in places within the Observatory inclosure on Calton Hill, in 1836, by Professors Forbes and Henderson, with a view to the question of the proper temperature of our globe—of what is the thermometric effect of the whole solar heat which falls in a year on the surface of the earth—how much is transmitted to the interior—how much dissipated at the surface—to what depth the influence of the seasons extends—

and in what manner that influence is modified at different depths. The registered observations, embracing a period from 1838 to 1854 inclusive, were ably discussed by my son, who concluded a laborious investigation of all the official documents with this remarkable peroration:—

Amidst many other remarkable features, one cannot but be struck, on inspecting the curves of the annual means, by the fact, that each of the four thermometers indicates on the whole a progressive increase of the mean annual temperature from 1838—1854.

What is the cause of this? If it be in the sun, the phenomenon is of the utmost importance, and demands continued and earnest attention. But does it arise from any slow alteration of the zero points of the thermometers? I have been assured by an eminent Cambridge author on the theory of heat, that there is no cause known likely to produce such a change; and there is also Professor J. D. Forbes' actual trial of one of the thermometers detailed at p. 229, tending to prove the same position practically. There is, too, a happy feature in the curve of the 12-feet thermometer, which makes it probable, and a happier feature still in that of the 24-feet thermometer, which renders it quite certain, that the epoch of our commencement, viz. 1838, was precisely at the bottom of a wave of temperature with about a sexennial period, and of which there are consequently three, all most distinct, within our period, 1838—1854.

Making, however, all due allowance for this circumstance, there still remains such a decided average increase of each thermometer for the whole time, that there certainly must be either a change in the instruments such as natural philosophers declare is altogether unlikely, but which it would be well that the Observatory had means for inquiring into; or, we have indications of the sexennial waves being but the periodical ripples of a larger secular wave in the development of solar light and heat; the summit of which wave may bring us years warmer than any that have been felt in our own days, and the bottom of it periods of cold in corresponding severity.

Here is an interesting conclusion; for, although Meteorology as a contributor to folk-lore and weather wisdom has revealed no end of good items, it must be confessed that as a science she has as yet been so stubborn as to appear hopelessly unapproachable. Still she evinces considerable sensibility under dexterous usage and reliable experiment: and it really appears that the "earth-thermometers" here treated of have actually obtained a concession, however slight it may prove. The majority of English meteorologists—as well as those of the continent—must well remember that, from about the year 1842, we had a succession of cold humid springs and stormy summers, with very few exceptions, down to 1855; and that since the latter date to the present epoch, 1859, we have experienced four seasons more characteristic of central France than England—

indeed the season which has just passed may be even rated at a higher temperature. From 1856 the Fahrenheit thermometer has reached 90° every summer; and, since 1857 inclusive, more than once. But in 1858 and 1859, I noted that it really and fairly attained to 94°, the instrument being in the shade. In a word, within the period thus named we have experienced the hottest summers, the hottest months, and the hottest days hitherto recorded by proper means; and to this the agricultural, floral, and horticultural worlds have all and severally borne unquestionable evidence. Weigh this well, and think!

It will readily be admitted, that much has been done towards investigating the constitution of our envelope and its extent, as well as that human intelligence has long been occupied upon some of the physical forces concerned in atmospheric phenomena: but a correctly true knowledge of the habitudes of the permanently elastic fluid, is still a most stringent desideratum—one requiring wide co-operation, and a full application of zeal and perseverance. We must here, however, quit this, and return to our more local subject.

The meteorological department at Hartwell has been placed under the care and diligent registry of Mr. Samuel Horton, who for some years past has furnished observations to the Registrar General of Births, Deaths, and Marriages, for publication in his official returns. An improved system of observing and recording the various atmospheric phenomena has been introduced here by Mr. James Glaisher, F.R.S. of the Royal Observatory at Greenwich; and the instruments now relied upon are as follows:—

I. The BAROMETER is a standard one, made by Barrow under the superintendence of Mr. Glaisher. It is of brass throughout; the scale is divided to $0^{in.}005$, and terminates in a conical point of ivory, which in observation is made to touch the surface of the mercury in the cistern, and the contact is very readily seen by the reflected and the actual point appearing just to meet each other. The vernier subdivides the scale divisions to $0^{in.}002$; it is moved by its rack and pinion, till the ray of light passing under the back and front parts of the semi-cylindrical plate carried by the vernier appears just to touch the convex surface of the mercury in the tube.

The tube is $0^{in.}32$ in diameter, the mercury has been boiled within it; the correction for the effect of capillary attraction is therefore $0^{in.}013$ to be applied additionally. The cistern is of glass. This instrument is recorded every day at 9 A.M. and at 3 P.M. At the top of the instrument are three screws, turning in the fixed part of the support for adjustment to verticality: and the readings of

the barometer are all too low by $0^{in}\cdot002$, as determined by Mr. Glaisher. All observations of this barometer are increased by $0^{in}\cdot013$ for capillarity, and by $0^{in}\cdot002$ for inside error, and they are also corrected for the difference of temperature of the mercury in the tube at the time of observation, from 32°, by the application of the corrections contained in the table for barometers whose scales are engraved upon brass reaching from the level of the mercury to the vernier. In practice one correction only is needed, a special table having been formed based upon the three necessary corrections.

II. The DRY BULB THERMOMETER is mercurial; its scale is divided to 1°. Its Index errors have been determined by Mr. Glaisher. This instrument is read every day at 9 A.M and 3 P.M.

II. The WET BULB THERMOMETER is mercurial; its scale is divided to 1°. It index errors have been determined by Mr. Glaisher. The bulb is covered with a piece of fine muslin, and near to it, but distant from the dry bulb thermometer, is placed a small cistern of rain or distilled water. A piece of cotton lamp-wick is connected with the muslin, and its end dips into the cistern of water; the water by capillary action ascends, and keeps the muslin on the thermometer constantly wet. This instrument is read every day at 9 A.M. and at 3 P.M.

IV. The MAXIMUM AND MINIMUM THERMOMETERS. The self-registering thermometer for maximum temperature of the air is mercurial, with a transparent bulb; its index is a piece of blue steel wire. Its scale is divided on box-wood to 1°. This instrument is read every morning at 9 A.M.

V. The MINIMUM THERMOMETER. The self-registering thermometer for minimum temperature of the air is of alcohol, with a transparent bulb; its index is glass, with a knob at each end. Its scale is divided on box-wood to 1°. This instrument is read every day at 9 A.M.

POSITION OF THE THERMOMETERS. At the distance of 58·5 feet south of the house, and on a grass-plot is fixed a post, which carries a revolving frame, similar in its construction in every respect with that at the Royal Observatory at Greenwich. This frame consists of a board four feet one inch and a half wide at the base, and of another board one foot eight inches high, connected with one edge of the horizontal board, and projecting upwards; and of two parallel inclined boards separated from each other by three inches, meeting the other edge of the horizontal board, and the top of the vertical board. Upon the face of the vertical board are placed all the thermometers in such manner that their bulbs project below the vertical board, and are about four feet above the ground. Above them is placed a projecting roof, to protect them from the effects of radiation and rain. The frame is always turned round after every observation, so that its inclined side is turned towards the sun.

VI. The RAIN GAUGE is a simple cylinder gauge, eight inches in diameter, and therefore having an area of 50·3 square inches. The height of the cylinder is thirteen inches and a half; at the depth of half an inch from the top, within the cylinder, is fixed a funnel (an inverted cone) of six inches perpendicular height: with the point of this funnel is connected a tube, one-eighth of an inch in diameter, and half an inch in length; a quarter of an inch of which is straight, and a quarter of an inch is bent upwards. By this arrangement, the last drop of water remains in the bent part of the tube. The upper part of the funnel, or base of the cone, is made to touch the internal part of the cylinder all round, and it is believed that evaporation is totally prevented. The cylinder is sunk eight inches in the ground. The quantity of water thus collected is carefully read at the end of every month.

VII. In addition to these instruments, a standard thermometer is placed on the revolving stand; so that, in the event of an accident happening to the dry bulb thermometer, the series of observations would be continued unbroken; and duplicate instruments of all kinds are kept for the same purpose, nearly the whole of which have been closely examined, and their several index-errors determined, by Mr. Glaisher.

On every day from the observations at 9 A.M. and also from those taken at 3 P.M. the true length of the column of mercury supported by air, and that portion supported by water mingled with the air, are determined, as well as the true temperature of air, evaporation, and the dew point; the weight of a cubic foot of vapour, the additional amount of vapour required to saturate a cubic foot of air, the degree of humidity, and the weight in grains of a cubic foot of air under its then pressure, heat, and humidity, are determined. All these hygrometrical results being calculated by the use of Glaisher's hygrometrical tables.

The average monthly and quarterly values of all these elements are determined, and they are published quarterly in the Reports of the Registrar General: and the whole of the instruments are frequently examined by Mr. Glaisher, who also examines all the meteorological work in progress.

Since this establishment was carried out, a well-made anemometer has been added to the mechanical force thus enumerated, in the hope of contributing towards the requirements which areometry now demands, by approaching the determination of winds with accuracy, so as to strangle the absurd method of registering, for science, such inconstant quantities merely by our own feelings. From the long prevalence of this practice, and the absence of instrumental aid, little hitherto has been advanced for the true ascertainment of the actual strength and velocity of the air. The efforts of Wolfius, Leutmann, Brice, and others, have not answered; but Edgeworth's mill principle, with the improvements of Robinson, Ostler, Whewell, Piazzi Smyth, and Harris, have lately brought anemometry * out into the fair-way of knowledge. The Hartwell machine is at once simple and portable: it is furnished with four hemispherical cup-vanes, which give motion by an endless screw to a train of wheels and pinions showing the number of revolutions made, on Massey's method with his well-known patent log.

* The Chevalier de Sade, in his "Tydologie," a work published in 1813, makes an angry predication of this harmless term, so expressive of its derivation, *ανεμος*, wind, and *μετρον*, measure:—"Les savans," he remarks, "pourront un jour appeler cette science *anemométrie*. Cette répétition monotone de syllabes sourdes et nazales, rappelle trop ce vers de Voltaire—

Non, il n'est rien que Nanine n'honore."

Another addition which Dr. Lee has added to the meteorological battery is one of the best Aneroid barometers which the art of Negretti and Zambra can produce, upon M. Vidi's principle of a metallic chamber partially exhausted; and a very beautiful—though not independent—instrument it is, for quickly indicating every variation of atmospheric gravitation, only that, even when at rest, the difference between it and the barometer gradually and irregularly increases, thereby denoting too high a pressure. The beauty and compactness of the Aneroid, aided by over-bearing advertisements, made it leap into favour *per saltum*, insomuch that many were induced to substitute it for the mercurial barometer; and this led me to examine it closely. The conviction arising from that scrutiny was so strong, that I warned travellers respecting its too hasty adoption from the chair of the Royal Geographical Society, at their Anniversary of 1851—being then the President; and the reader will find an extract from my Address in the Appendix to this volume, No. VI. But while I consider that for certain important scientific purposes this instrument was greatly overrated, and therefore mischievous, I readily admit that it is an admirable weather monitor, and remarkably well adapted for general nautical uses, because it is less sluggish than the marine barometer in its indications, and more ready to read under violent motion.

A third additament consists in noticing the hues of test-tickets, or ozonometers, which are strips of papers chemically prepared by Negretti and Zambra, of Hatton Garden, London, on Dr. Moffat's new formula, for the detection and strength of *ozone* in the atmosphere. This is a singular aerial principle discovered by Dr. Schönbein, who described it to me as an odour derived directly from electricity: but as to its actual presence, and effects, we have yet to multiply observations to a great extent, before hints can be converted into certainties. The present manner of ascertaining whether it exists, is by putting a slip of the test-paper in a place freely exposed to the air, but protected from light and atmospheric currents; it is then compared with a prepared scale and registered, but the form can only as yet be deemed conventional, and the relative values of precarious ascertainment. The difficulty of theo-

rizing upon such scanty and delicate data ought not, however, to impede the continuance of observing the facts.

During eleven years of the time that I was employed in the Mediterranean, I carefully kept a very regular account of the fluctuations and general state of the barometer, sympiesometer, thermometer, and hygrometer, with, of course, a due registry of the winds; and the conclusions I arrived at may be seen in my Memoir on that classic sea. In order to draw more attention to the subject than it had received in some quarters, I wrote several papers on Meteorology, and sent them in 1829 to the United Service Journal—a sort of Naval official medium—to enforce attention; besides which I furnished that periodical with a monthly return of the maxima and minima of the atmospheric pressure, the temperature, rain, evaporation, and winds, for upwards of another space of eleven years. In 1830, I urged the necessity of its being attended to in our public observatories, in my account of the Bedford establishment read to the Meeting of the Astronomical Society; since which a wonderful change for the better has actually taken place (see Cycle, I. p. 344). But, though thus earnest in the cause, I never contemplated that mere readings of graduated scales, or watching the vagaries of a needle, should impinge upon the higher duties of astronomy, or claim anything approaching to equal rank. There can be little doubt but that great accuracy and value has been introduced of late by the system of simultaneous meteorology; and those who discuss hypotheses require the aid of intelligent observers of facts. But, while rendering due acknowledgment to all true contributors to our further acquaintance with weather-wisdom, we cannot recognize as belonging to that very useful body those who merely clog us with indifferent records—to the dismay of those on whom the task of discussion falls. It may be true that the fluctuations of our atmosphere are governed by definite laws, but these laws will not be detected by such assistance. Then again, when we ponder on the rights filched from Urania by the photographic registration of passing aerial phenomena, however ingenious its automic application, who but must regret the expence in cash, time, and trouble occasioned by introducing it into an astronomical

observatory? Here, to say the least, it must inevitably damage the more important and integral power for carrying out the intentions for which the institution is founded—unless indeed it may be at such a place as Greenwich, where there is, or can be, a sufficient force to allow of divided work. But, after all, I have watched in vain for the photogenic process—involving a continual outlay in apparatus, chemicals, gas, and garniture—bringing to light a single fact which might not have been cheaper, and as accurately, done by the eye. Those bound folios of serrated botches now on the shelves of my diligent friend, not 100 miles from here, are doomed to a moral dry-rot: we fear the process is mostly a mere fuss without business—"great cry and little wool." 'Tis but an ingenious philosophical toy at present.

Meteorology is also cruelly impeded by the seers of weather almanacs, astrological annuals, and the like, who, enjoying an oblivious irresponsibility, very shamelessly propagate misleading nonsense; and

"A rousing whid, at times to vend,"

with their trash, have not scrupled to "nail't wi'" well-known and honoured names. Thus Halley was fraudulently made use of, Hook was constantly a scapegoat, and Newton himself was not exempted: but the most pertinacious attack of this kind is, perhaps, the long-continued slander upon the late Sir William Herschel. This meritorious philosopher, in common with all men of true knowledge, was well aware of the folly of making weather-predictions under supposed lunar, cometary, and planetary influences, the which can never be recognized as being under the cognizance or tutelage of Astronomy. Yet some hungry wight—relying on those who according to Solomon are infinite—drew up a prophetic table of meteoro-astrological presages—or, rather, pirated it from the erudite *Curiosiora et Selectiora Variarum Scientiarum Miscellanea* of Martin Szent-Jvany. This sapient document, cooked afresh, was served up and widely circulated as a preparation of Herschel's; and it was as such eagerly received in numerous quarters despite of the indignant defendant's exerting his utmost to contradict it in the public newspapers. One of disavowals was inserted in the *Philosophical Magazine* (vol. xxxiv. page 237), and is thus couched:—

"Many of the public papers, for a length of time past, have occasionally ascribed certain predictions of the state of the weather to me; and several of them have lately gone so far as actually to prefix my name to what they have called a 'Weather Table,' in which, according to certain changes of the moon, wind, rain, snow, frost, &c., are prognosticated. Such a table, by some mistake, has even been very lately inserted into a very respectable philosophical magazine. In justice to myself, therefore, I think it highly necessary that the public should be undeceived, by my declaring that the table pretended to be of my construction, as well as every prognostication of the state of the weather that has appeared in the newspapers, as ascribed to me, are all gross impositions.

"WILLIAM HERSCHEL.

"*Slough, near Windsor, Sept.* 16, 1809."

Now any person would have thought this honest statement would have proved a sufficient vindication and caution; but no, the weed had taken root, and, like the recent portentous treatise called VESTIGES OF CREATION, it was repeated *ad nauseam*. Even now there lies before me the actual letter which that great astronomer wrote, five years after the above one, to Mr. Leigh, the eminent publisher; and from this the annexed plate is copied *per factum simile*.

All efforts to suppress or even abate the nuisance were unavailing, for the divination was copied and re-copied in every direction; and even the staid editor of the Naval Chronicle was imposed upon to buy and insert it, as having been constructed by a naval officer (XXV. page 328), the work of "many years actual observation, from a philosophical consideration of the attraction of the Sun and Moon in their several positions respecting the Earth. Its object is to indicate, on a simple inspection, what sort of weather is likely to succeed, according to the changes of the Moon, either in the summer or winter. Our correspondent informs us, that it is so generally correct as seldom to have been known to fail." Laud we the Gods!

During the Murphy weather-mania, Sir William was again dragged out into print, on which I sent the above-cited denial to my friend Major Clerke, editor of the United Service Journal, wherein it was inserted (Part I. of 1835, pages 102-4), for the edification of all the anti-Newtonians afloat, and as a warning to the better inclined. That error which proceeds from an involuntary deviation from truth is easily dealt with; but error prepense is a wily serpent, quick in gliding about, and difficult to strangle even when caught; for, after all the exposure above given, I was told lately by a scion of the addle-

Mr Leigh
Bookseller
No 18 Strand
London

WINDSOR

Sir

I am glad of an opportunity to say that prognostications of the weather are so much above the knowledge of astronomers that I have taken uncommon pains publicly to contradict reports of predictions that have been ascribed to me. You may therefore be assured that what you have heard as my opinion about the frost is without the smallest foundation. If at any time Slough should be in your road, I shall be very glad to see you here, and remain

Sir

your most obedt
Servt

Wm Herschel

Slough
near Windsor
Feb 6. 1814.

headed school—"I find Herschel's rules on the weather always correct." But there is no end of the matter—in December 1841, Sir John Herschel himself was compelled to take the field and write a missive to the editor of the *Vienna Economical Calendar*, in these terms:—

It is singular how greatly prevalent the opinion is, that both my father and myself have advocated the idea of the Moon's influence on the weather, and published predictions and weather tables founded on such presumed influence. But there is not the shadow of foundation for any such idea; on the contrary, all possible pains have been taken on the part both of him and myself to disavow and disclaim all such pretended tables and predictions. The only way in which I have ever connected the Moon and the weather is by a casual remark, that I very frequently observed a full or nearly full Moon (*i. e.* sensibly *round*) to be accompanied with a sky serene and cloudless, or nearly so, at or within an hour or two after its rising, so often, indeed, that, though I do not pretend to see any rational connection, I cannot help thinking that there is something beyond accident in it, and have therefore recommended it to meteorologists as worthy of some notice for the future. —(*See ante*, pages 59-60 of this volume.)

Surely such pertinacity in a wrong cause must originate in the unwholesome weakness alluded to in the Cycle, (I. pages 21-4,) for otherwise the belief in astral influences is unaccountable. The erudite divulgation there mentioned, ZADKIEL, is already advertised for 1860; and the intelligent public are informed that it will appear, "With a large Hieroglyphic of a warlike description, the Nativity of Louis Napoleon for some years to come, &c." Assuredly such inane and damaging crudities are unworthy of the age which so boastfully glorifies itself before all others as having achieved a very wondrous march in intellect, and ought not to find a single reader who is capable of gazing upward—ἄνθρωπος—who alone of all creatures escapes the doom of continually facing the earth—

Pronaque cùm spectent animalia cætera terram,
Os homini sublime dedit, cœlumque tueri
Jussit, et erectos ad sidera tollere vultus.

In the outset of science, the fluctuations that occur in the lower atmosphere might seem to depend on the motion of the planets, which are ever shifting their berths; and stellar influence pervaded the meteorological creed of the vulgar. But surely the truths of astronomy, together with the results of philosophical inquiries into the constitution of the globe's elastic envelope, should deal a *coup de grâce* to such hap-hazard notions and frivolous absurdities.

L'Envoy.

Here closes our mite towards the grand fund for exploring and explaining some of the more palpable mysteries of the skies, usually included under the sage category—secrets of nature. The probing into these may, for a long time to come, be restricted to the collecting of facts hereafter to be connected with physical laws demonstrating their structure and arrangement, and proving throughout the wondrous adaptation of means to their ends. Assuredly the object of investigation is supremely glorious, and rich is the harvest which yet awaits the precise observer, who, fully alive to the marvellous scenes around him, is prudently callous to the delusive theories of rabid enthusiasm.

The Cycle of Celestial Objects, my former astronomical work, sufficiently explains what the Bedford Observatory was; and the present volume, it is hoped, shows the rise, condition, and prospects of that of Hartwell—the last being, so to speak, a pet fondling of the former. Several other offsets of the same stock have also attained maturity, and may success attend them, for it is honourable to England that she has taken the lead of the world in these praiseworthy private establishments; the which, while they act mildly as an incitement to our public observatories, are each likely to polish some particular scientific gem for the accretion of mental perception. Moreover, besides the positive utility of making, reducing, and discussing heavenly observations, surely nothing can contribute more to the elevation of thoughtful reasoning, and the intellectual intelligence of man, than the contemplation of the wonders revealed by the Omnipotent; albeit our perceptions can only trace by faint shadowings the vastness of his power and glory. Much is certainly now known; but higher views are in all probability attainable, if the various ramifications of knowledge are perseveringly and properly pursued: for it is, in the present day, still more obvious than it could have been to Pope, that—

All nature is but art unknown to thee;
All chance, direction which thou canst not see;
All discord, harmony not understood;
All partial evil, universal good.

And now, studious reader, gaze attentively on the appended means of comparing the several bodies of our own system only—reflect on all the investigated phenomena which gravitation, controlling the like laws from Mercury to Neptune, has produced on their forms and motions by mutual actions on each other—and then boldly ask, What human mind dare limit the mechanism, or scan the design, of the material universe?

APPENDIX.

NO. I.

STORY OF THE NEW PLANET, NEPTUNE.

(*Referred to in page* 86.)

"What if the Sun
Be centre to the world; and other stars,
By his attractive virtue and their own
Incited, dance about him various rounds?
Their wandering course now high, now low, then hid,
Progressive, retrograde, or standing still,
In six thou seest: and what if seventh to these
The planet Earth, so stedfast though she seem,
Insensibly three different motions move."

Thus sung Milton in the middle of the seventeenth century, when Astronomers were contented with six primary planets, besides the hypothetical globe which was held by mankind to be the centre of the system: but since that period, no fewer than seven more of these revolvers have been detected, and 'tis a joint-stool to Cassiopea's chair that others will still be found.* Thus the recent triumph of science in the *à priori* pointing out of such a body, because certain twitchings in the motions of Uranus indicated an exterior attraction, is an advent so unexampled as to form one of the most brilliant achievements of intellect ever recorded, and to raise prospects of future harvests. Its story must therefore be placed on our pages.

It will be recollected that Kepler, looking to the general harmony of the celestial spaces, and the obvious analogy which existed in the distances of the primary bodies from the Sun, had already confidently predicted the discovery of a planet between Mars and Jupiter. This notion, though not resulting from any known law of nature, seized upon men's minds. Lambert considered another body as vitally necessary in the construction of the universe; and Bode, in 1772, found that some-

* Between the time when this prediction was published (1847) and the present year (1859), no fewer than 51 small planets have been added to our system!

thing was required to make the table, which he had analogically constructed, accommodate itself to the system. The actual discovery of Uranus nine years afterwards aroused attention to the hypothetical subject; and in 1789 Baron de Zach confidently published, in the Berlin Almanac for that year, the elements of the orbit of the yet undiscovered planet which ought to be found in the vacant space, making its assumed distance from the Sun 268 millions of miles, and its period 4 years and 9 months. In 1800, an association of 24 astronomers was formed for prosecuting the search by observing every visible zodiacal star. After the discovery of Ceres in 1801, having nearly this very distance and period, Professor Bode communicated his celebrated empirical law of the planetary system, above alluded to, to Baron de Zach. This simple *law* is so far a mere experiment, that no rational ground or physical theory can be adduced in its favour, nor is it capable of mathematical demonstration; and yet it has had the proud triumph of achieving wonders in respect to the Asteroid family, and has now formed the basis of the heliocentric distance assumed for the new exterior planet. The scheme teaches, that the interval between the orbits of any two planets is about twice as great as the inferior interval, and only half the superior one. In other words, the excess of the distances of the planets above Mercury form a geometrical series, of which the common ratio is 2. Now, if the radius-vector of Mercury is represented by 4, then that of the other planets will follow the annexed law of distance, increasing by 3×2 and its powers; and this very ingenious relation—which certainly has proved helpful—is thus marshalled:

Mercury	4	=	4
Venus	$4 + 3$	=	7
Earth	$4 + 3 \times 2^1$	=	10
Mars	$4 + 3 \times 2^2$	=	16
Ceres (as mean of Asteroids)	$4 + 3 \times 3^3$	=	28
Jupiter	$4 + 3 \times 3^4$	=	52
Saturn	$4 + 3 \times 2^5$	=	100
Uranus	$4 + 3 \times 2^6$	=	196
Neptune	$4 + 3 \times 2^7$	=	388
Hypothetic Planet	$4 + 3 \times 2^8$	=	772

and so on to the 10th power—the very outskirt of the cometary aphelia—beyond which there can be no hope of a visible body. And it is not a little singular, that this most accidental tabulation should yield nearly the correct relative heliocentric distances of the primaries of our system: for though mathematically it has proved inexact, the curious contrivance was found to be of material and ready use as the probable expression of a general reality, in hypotheses of solar radii. It is otherwise thus expressed—say the space between Mercury and the Sun $= a$, and the distance from Mercury to Venus $= b$; then the whole planetary series may be thus represented—

$$a;\ a + b;\ a + 2b;\ a + 4b;\ a + 8b;\ a + 16b;\ a + 32b;\ a + 64b;\ a + 128b.$$

To be sure, the newly-found planet seems to be six or seven solar radii short of what Bode would give; but it must be recollected that something may still be shown for this, there being certain little items remaining for philosophy to ponder upon. The mean distance, from a very rough view of the few observations hitherto taken, may be under 32 of those radii: but it should be

borne in mind that the empirical distance used led to the grand discovery which we now treat of. At all events, the said distance is not so violently out, but that deeming it an utter failure is *troppo duro;* for, though in strictness it must be said to have miscarried, and is therefore thrown overboard, still it must be acknowledged that, unfurnished with this law, the astronomers and geometers who were rowing together, would have been at sea without a compass to steer by. It is said that Titius, a Wittemberg professor, gave Bode the first notion of the clever conjecture.

Few of our readers are unacquainted with the *empirical,* though philosophical discovery of the Asteroids, as they were prepensely sought for in a region where, according to the best guesses, a planet ought to have existed; and where, upon a singularly strict and methodical scrutiny, four little planets were actually found—Ceres, Pallas, Juno, and Vesta—which, from several very receivable evidences, have been inferred to be mere splinters of a large body which has undergone some tremendous catalysis. They all conform to the same order of distance—two of them with almost the same periodic times—but the orbits of all of them, so far as known, intersecting each other in a very unusual manner. This is startling, seeing that every other distance on the scale of the Solar System gives but one primary orb, albeit there are satellites in four of the cases. Lagrange and Olbers thought that such ruin might be effected by a force with a velocity only 20 times that of a 24-pound shot on first leaving the mouth of a gun. From this great probability of a ruined planet, it was considered that other fragments would still be occasionally picked up; and accordingly Herr Hencke, of Driessen, in Prussia, on the 8th of December, 1845, discerned a fifth small asteroid belonging to the same group, which has received the name of Astræa.

Great, however, as was the triumph of mind and means which led to the discovery of the new planetary family, the finding of the Asteroids has just been so greatly transcended, that it must now occupy a secondary niche in the temple of Physical Astronomy. By methods depending on the furthest advance ever yet made in theoretical and practical thought and science, a new body of the Solar System has been brought to light; and, while the wondrous tidings are still ringing in our ears, a few words may be acceptable, even though we may perchance pay out a little stray line.

The doctrine and admirably successful application of mathematical analysis to the profound study of celestial phenomena, have opened and matured the perfection of the scientific character of Astronomy, and the preponderating importance of the fixed correspondence of cause and effect which it unveils. This surprising mechanism, exhibited by the laws of the planetary motions, may be justly considered as the grand foundation of the whole system of positive knowledge: and yet, in spite of appearances, the phenomena are, in fact, much more consecutive than would be thought. The most complicated problem which they present—that of the modification produced in the motion of two bodies tending towards each other by their mutual gravitation, from the influence of a third acting on both in the same manner—is much less complex than some other portions of inorganic physics; and yet it presents such difficulties that the solutions of it are still only approximative. The Solar System has presented certain facilities for advancing to perfection in the unravelling of its several problems; and each hypothesis admits of numerical verification. The planets of which this system is composed being few in number, having their masses very unequal, and incomparably smaller than that of the sun: their forms being almost spherical, with their orbits slightly inclined towards each other and nearly circular: from these circumstances, it results

that the perturbations are often very slight, and that in order to calculate them it is commonly sufficient to take into account, concurrently with the action of the Sun upon each, the influence of one other planet, capable, from its magnitude and proximity, of producing sensible derangements. These disturbances had heretofore given philosophers no small uneasiness, because they were not understood; and, from what was obvious to the mere senses, there appeared to be a general tendency in the planetary bodies of the Solar System to descend to the common centre of gravity of the whole. Now, under the potent subjunctive, *if* this were true, it would follow that the material universe could exist only for a few millions of years longer. Such a catastrophe was truly alarming in anticipation; and, in the general dismay and consternation which it occasioned, the tuneful bard of the Botanic Garden thus dolefully sighed—

> "Roll on, ye stars, exult in youthful prime,
> Mark with bright curves the trackless steps of Time;
> Near and more near your beaming cars approach,
> And lessening orbs on lessening orbs encroach.
> Flowers of the sky! ye too to age must yield,
> Frail as your silken sisters of the field!
> Star after star from heaven's high arch shall rush,
> Suns sink on suns, and systems systems crush:
> Headlong extinct to one dark centre fall,
> And Death, and Night, and Chaos mingle all!"

But hope was quickly at hand; the conditions of the case were closely scrutinized, and the geometer, extracting a gratifying destiny from his formulæ, chanted a palinody, and declared Nature to be immortal! In short, the noble brace of *compatriotes*, Laplace and Lagrange, after submitting the celestial perturbations to the test of the extraordinary resources yielded by the Differential Calculus, went far to demonstrate the eternal duration of the universe: in fact, they fully showed that the effects of pure gravitation-disturbances in the Solar System, went on compensating one another. Nor was this all: the law of those changes, which prescribes to each its limits, and forces it to recur in endless repetitionary series, was clearly established. This was extremely satisfactory on the whole; but there are still misgiving inquirers who think that a resisting medium, and other natural practical difficulties which may exist, are not taken account of in the mathematical theory of gravitation. This, however, is but a mare's nest as yet.

Such, then, is the general argument. All the planetary motions are affected by the gravitation of the planets towards one another; and their places in the heavens are computed beforehand, so that the positions given by observation can be constantly compared with those previously calculated. This led to the recent extraordinary revelation. The observed motions of Uranus, the most distant body hitherto known in our system, when thus compared, were found not to agree with those which it ought to have had after allowing for the influences of all the known planets. In 1821, Mons. Alexis Bouvard published his Tables of Uranus, founded on a continued series of observations extending from 1781, the year in which the elder Herschel detected it, to the date of his publication. Previous, however, to its discovery as a planet, it had accidentally been observed no

less than seventeen times as a fixed star, the earliest being in 1690, by Flamsteed. When Bouvard was constructing his tables, he attempted to form them as well on the older as the later observations, but, whatever ellipse he ascribed to Uranus, he could not make the theoretical agree with the observed course of the planet; and, thinking these discrepancies might be owing to imperfection of method and means in former days, he rejected all the observations previous to 1781. It was afterwards found, however, that the rejection had been made on insufficient grounds, and that, from some unknown cause, the theory itself was in fault, the difference amounting to no less a quantity than 96″ of geocentric longitude in 1841. This increase had accelerated since 1833, for as early as the year 1838 Mr. Airy, on a comparison of his own observations with the Tables, found that the planet had yawed from its computed course by a quantity nearly equal to the distance of the Moon from the Earth; and that it was actually describing an orbit greater than that pointed out by theory. When it was thus found that the deviations were far greater than any which could be ascribed to mere errors of observation—that they were of a regular character—that they were in the same planetary direction—and were of such a nature as would arise from the action of a still more distant planet, attention was naturally directed to inquire whether the refractory disturbances were such as to afford a clue, which might possibly lead to the location of the invisible disturber, whose attraction was thus influencing the progressive motion of Uranus, and dragging it away from the Sun. This —after much labour and many misgivings—betrayed the attractor; for it seemed quite clear that, without a departure from the received law of attraction, or the actual existence of a massive disturbing body, there was no accounting for the irregularities under notice. The whole, however, was held to be a mere transient hypothesis until within the last handful of years; and this brings us to our eventful story.

Among all the physical sciences, that which investigates the laws of the celestial bodies, and extends man's views to other spheres, is unquestionably the most beautiful effort of the human mind: and it follows that the recent installation of Neptune—one of the largest planets of our system—is among the noblest feats of Astronomy. This we confidently assert, since the predicting and finding were not the fruits of accidental fortune, persevering scrutiny, or telescopic power; but the magnificent consequence of a judicious exertion of profound thought and transcendental reasoning. Indeed, in the whole history of science, there is no event more striking than this, and its circumstances are so interestingly curious, that we beg our readers' close attention to all the leading features of the case.

The Astronomer-Royal, having been somewhat officially made acquainted with many of the leading circumstances, gave a succinct historical detail of them at the meeting of the Royal Astronomical Society of London, on the 13th of November, 1846. Throughout this interesting discourse, there is internal evidence of the strictest truth, and it furnishes a mass of successive facts, which no mere opinions can rebut. He commences with observing, that the irreconcilability of the motions of Uranus with the law of gravitation gained general credence from the publication of the French *Tables of Uranus* in 1821; and he afterwards adds—"I know not how far the extensive and accurate calculations of M. Eugène Bouvard may have been used in the subsequent French calculations, but I have no doubt whatever that the knowledge of the efforts of M. Bouvard, the confidence in the accuracy of his calculations, and the perception of his failure to reconcile in a satis-

factory way the theory and the observations, have tended greatly to impress upon astronomers, both French and English, the absolute necessity of seeking some external cause of disturbance." In November, 1834, Mr. Airy received a letter from the Rev. Dr. Hussey, who had recently visited Paris, in which he remarks on the possibility of a disturbing body outside Uranus; but adds that he "found himself totally inadequate to the task" of hunting it up, so as to determine its position even approximately, or empirically. He therefore relinquished the matter altogether; but on mentioning it to Bouvard, that astronomer said it had occurred to him also*—and that Hansen conjectured there were two unseen planets in the case, as it seemed clear that one disturbing body would not satisfy the phenomena.

This is the Astronomer-Royal's first point of departure; and the act of publishing his own reply, in which he doubts the possibility of making such a determination, is creditable to his candour. As, however, straws thrown up often serve to show which way the wind blows, we are surprised at the omission of many other existing hints on the subject, for, even dubbing them only *fancies*, they all tend to show that the discovery of Neptune is fairly a result of the march of thought, consequent on the heave-ahead movement of the age. Now, everybody recollects that Immanuel Kant predicted, or rather demonstrated, that a planet exterior to Saturn would be found, as a deduction from certain laws which he had established in his *Himmels System:* for he suspected that, as nature does not ordinarily proceed *per saltum*, the system of planets must pass by *gradation* into the system of comets. Therefore, he asserted, at some future period there will be found *at least one planet* superior to Saturn—whose orbit will be much more excentric—and will thus supply a link to connect the motions of the planets and the comets into a more continuous chain. This moral, rather than mathematical, decision was given at least twenty-six years before Herschel made his discovery. In 1758, Clairaut also supposed the irregularities of Halley's comet might be owing to planets too distant to be ever perceived by us; and a floating notion to that effect is traceable elsewhere. There is a passage somewhat in point in J. G. Jacobi's Pocket-book for 1802, which runs thus—"Ophion, the next planet beyond Uranus, is 780 millions of miles distant from the Sun, and has an orbit of 250 years. It is not yet discovered." Signor Niccolo Cacciatore, of the Palermo observatory, must have had notions of a similar stamp, since we find him writing to us in 1835, that he had watched a *suspicious* star near No. 17 Hora XII. of Piazzi's Catalogue, and found a movement of 10″ in right ascension in three days: "So slow a motion," he said, "would make me suspect the situation to be beyond Uranus."†

Mrs. Somerville, in the third edition of her *Connexion of the Physical Sciences*, 1836, at page 74, says:—"The tables of Jupiter and Saturn agree almost perfectly with modern observations; those of Uranus, however, are already defective, probably because the discovery of the planet in 1781 is too recent to admit of much precision in the determination of its motions, or that possibly

* In 1837, Eugène Bouvard wrote to Mr. Airy,—"Cela tient-il à une perturbation inconnue apportée dans les mouvemens de cet astre par un corps situé au-delà? Je ne sais, mais c'est du moins l'idée de mon oncle."

† It having been publicly advanced that this stranger and La Verrier's might prove to be identical, we beg to remind the reader, that the latter has only 16° of motion in 10 years; whence, from its longitude in 1835, it was then in the xxth hour, or 120° from the scene of Cacciatore's phenomenon.

it may be subject to disturbances from some unseen planet revolving about the Sun beyond the present boundaries of our system. If, after a lapse of years, the Tables formed from a combination of numerous observations should still be inadequate to represent the motions of Uranus, the discrepancies may reveal the existence, nay even the mass and orbit, of a body placed for ever beyond the sphere of vision." At a public lecture delivered in February 1840, Bessel stated—"I have arrived at the full conviction that we have in Uranus a case to which La Place's assertion, that the law of gravitation entirely explains all the motions observed in our solar system, is inapplicable. We have here to do with discordances whose explanation can only be found in a new physical discovery. Further attempts to explain them must be based upon the endeavour to discover an orbit and a mass of some unknown planet, of such a nature that the resulting perturbations of Uranus may reconcile the present want of harmony in the observations." And in 1841, Herr Mädler published his excellent volume *Populäre Astronomie*, in which—discussing the probability of finding new planets—he says:—

"Finally, there remains the unlimited space beyond Uranus. The domain of the Sun extends at least forty times farther, for comets such as that of 1680 attain this distance in aphelion, and many of those whose orbits can only be now reckoned as parabolas, may extend considerably beyond. From what we know of the distance of the fixed stars, the nearest is distant several hundred thousand times that of the Earth from the Sun: therefore a planet whose distance is only two thousand times that of the earth (= 100 × ♅) would not incur a perceptible perturbation from the nearest fixed star, should this not much exceed that of the Sun. The existence of still more planets beyond the Uranian orb, is, for these general reasons, already very probable.

"Special reasons may be added in support of this probability, the most important of which is the following:—

"If a planet revolves beyond ♅, it must (if its mass be not very small) act on ♅, producing anomalies in the latter's course, inexplicable unless the disturbing body be known. Of course these anomalies would only appear after a considerable time, for, by reason of the ♅ period of 84 years, and the still longer one of the disturbing planet, these effects remain nearly the same for a long series of years, are mixed up with the elements deduced from the observations of those years, and are practically inseparable from them. But calculating a considerably earlier series, *other* elements would result, and one cannot combine both series in one system of elements without leaving notable errors. This is really the case with ♅. Bouvard (Tables Astr. de ♃, de ♄, et de ♅, Paris, 1821) found the pre-Herschelian observations incompatible with those elements which the more numerous observations from 1781 to 1820 gave, and which we must assume, on account of the latter ones being preferable. The deviations are by no means so great as to make us doubt the observed body having been ♅, but too great to be reputed errors of observation on the part of such careful astronomers. Moreover the observations after 1820 deviate considerably from Bouvard's Tables. Airy has shown from the oppositions of 1833 to 1837 that ♅'s radius-vector for these years differs from the Tables by a quantity exceeding the distance of the Moon from the Earth, and therefore certainly the last twenty years, calculated by themselves, would give quite a different system of elements from the preceding forty years. Did we *not* take into account ♅'s perturbations of ♄'s orbit, or ♄'s of ♃'s orbit, we should find similar deviations, and had we possessed *very exact* ♄n observations for a long series of years, it would have been possible to have discovered ♅ *theoretically* by analytical combinations before Herschel found it, presuming that all the disturbing masses were sufficiently well known and introduced in the computations.

"Applying this conclusion to a member beyond ♅, we approach (es liegt nun nahe) *a planet acting upon and disturbing it*; yes, we may express the hope, that analysis will some time or other solemnize this her highest triumph, making discoveries with her *mind's* eye in regions where our actual sight was *unable* to penetrate.

"Still the latter is not absolutely true. Although ♅ is considerably fainter than the other old planets, it is yet far from the extreme limit of visibility; a sharp naked eye may perceive him. A planet, of which the brightness would be to ♅'s as ♅'s to ♄'s would not be the faintest object in great telescopes, especially if its diameter be not too inconsiderable. The small orbital inclination of the three exterior planets to the Ecliptic, renders it probable that the one or more to be discovered do not exceed them by above 1° or 2°, and a few years' continuous investigation of the Ecliptic and its nearest limits, would be at first the most probable means of realizing this hope, or otherwise of showing that there is no planet beyond ♅ to which the optical power applied was sufficient."

The author then proceeds to Wurm, and to Bode's law $a+b^{n-1}c$, to which he however attaches little credit, and says that, although this law might have prevailed primitively, the *present* Solar System requires perhaps the introduction of the masses, inclinations, and excentricities: the latter being subject to secular variations, and our being ignorant of the *age* of the Solar System, render Bode's series imperfect; if true, we should have $a_8 = 38{\cdot}8$, $T_8 = 243$ years; $a_9 = 77{\cdot}2$, $T_9 = 7$ centuries, &c. He speaks of its having anticipated the Asteroidal discoveries, and concludes with the circumstance of the place of the aphelion of the comet of 1680 from Bessel's calculations.

(Table p. 346, No. $36 = 2 \times 426{\cdot}6858 - {\cdot}00622236 = 853{\cdot}3654$; $T = 8813{\cdot}782$ years) allowing of five planets, the last of which has $a_{12} = 620$, $T_{12} = 15000$ years.

In the cruelly-murdered Bailly's *Traité de l'Astronomie Indienne et Orientale*, 1787, page 203, there is a pointed reference to a Brahminic diagram, representing the orbits of the seven planets surrounded by two circles called *Natchatter* and *Akash;* and he lauds the Indian simple and rational theory as yielding astronomical tables which—in the absence of Copernicus, Kepler, and Newton—are pretty passable. Now may not these two circles outside Saturn—instead of a *primum mobile*—symbolize Uranus and Neptune? As it is within possibility that they may have been visible in an oriental sky, their existence might have been inferred by the early Hindoo astronomers.

Such hints as to a Trans-Uranian planet may be deemed vague and indefinite, and the conjectures, being unsustained by any show of reasoning, were of no scientific value; yet they ought to have been cited at a general muster, as bearing on the upshot; and there is still a point of greater interest and exactness, which should have been noticed. Early in the month of July, 1842, the illustrious F. W. Bessel—above quoted—visited Sir John Herschel at Collingwood, when, among other matters, he remarked that the motions of Uranus, as he had satisfied himself by careful examination of the recorded observations, could not be accounted for by the perturbations of the known planets; and that the obliquities far exceeded any possible limits of error of observation. "In reply to the question," says Sir John, "whether the deviations in question might not be due to the action of an unknown planet?—he stated that he considered it highly probable that such was the case,—being systematic, and such as might be produced by an exterior planet. I then inquired whether he had attempted, from the indications afforded by these perturbations, to discover the position of the unknown body,—in order that a *hue and cry* might be raised for it. From his

reply, the words of which I do not call to mind, I collected that he had not then gone into that inquiry; but proposed to do so, having now completed certain works which had occupied too much of his time." A few days after this visit to Herschel, at a breakfast with us in London, when we expatiated on the beautiful coincidence of lively and even fanciful theory with industrious practice afforded by the Asteroid plot, he remarked—"There may yet be something still more so,—for what if another planet shall be found outside Uranus?" We replied that, should it observe Bode's law, it would be too distant for vision. "That," rejoined he, "must depend on its volume." With a view of attacking this grand problem, on his return to Germany, he engaged the services of M. Flemming, to make all the necessary reductions of the available observations of Uranus; the labour was well advanced when Flemming died, and Bessel was attacked by the fatal illness which interdicted every abstruse mental application.

We have now approached the time when analytical investigation led to a distinct indication of the place where the disturbing body ought to be sought; and the wondrous efforts by which it was accomplished are confined to two geometers—Le Verrier and Adams; in merit *æquales*, and, all but in circumstance, both imperishably associated in this great intellectual achievement. We will proceed with the former, as his views were first published to all Europe, and his operations claim, therefore, priority;—merely premising that each, in ignorance of the other's labours, proceeded to investigate this most intricate question, and arrived independently at all but the same conclusions, for both pronounced that the probable place of the suspected planet was about 325° of heliocentric longitude! At the time of their undertaking this almost hopeless investigation, neither of these choice spirits had attained either the age or the scientific standing to raise expectation: yet they solved the stupendous problem with a completeness that surprised the veteran astronomers, and demanded the admiration of the whole intellectual world.

In the *Comptes Rendus* of the French Academy, 10th of November, 1845, there is a most valuable Memoir, by M. Le Verrier, on the perturbations of Uranus produced by Jupiter and Saturn; and on the errors of the elliptic elements of Uranus, consequent on the use of erroneous perturbations in the treatment of the observations. Herein the secular inequalities of those superior bodies are more rigidly investigated than ever, and many small terms—*un certain nombre de petits termes qui n'avaient pas été donnés*—added to the conditions; but on a laborious examination of the correction for the new elements, it was found to be incapable of explaining the observed irregularity of Uranus. "Cette discordance," he says, "préoccupe vivement les astronomes, qui ne sont pas habitués à des pareils mécomptes. Déjà elles a donné lieu à un grand nombre d'hypothèses. On est même allé jusqu'à mettre en doute que le mouvement d'Uranus fût rigoureusement soumis au grand principe de la gravitation universelle." And he sums up:—"Tel est effectivement le sens de l'erreur des Tables actuelles. Seulement, l'écart est plus considérable, et le surplus peut tenir à d'autres causes dont j'apprécierai l'influence dans un second Memoire."

The promised memoir was published in the *Comptes Rendus* for 1st June, 1846, and, though rated by its author only a sketch—*recherche*—was received with delight and astonishment by the scientific public, who seemed to partake of the geometer's zeal and confidence. M. Le Verrier re-considers all the possible conditions of the wandering between theory and observation in the orbit of Uranus, and, on grounds of the strictest geometrical reasoning, concludes that none of the explanations offered — as the resistance of ether, decay of gravitating power, action of a vast

satellite, or the shock of a comet—are admissible, save that of an unknown planet. On this supposition he showed that if this be the cause of the disturbing force, the stranger must be, not *within* the orbit of Uranus, because, if a large body, it would trouble the path of Saturn also; if a small one, it would be inadequate to produce the discerned effects; nor, for the same reasons, near on the outside of Uranus; but far enough *without* the orbit of the latter to act upon it without acting upon that of Saturn; and large enough to act upon Uranus for long and continuous periods of time. The great question now was—where then is this body situate, what is its mass, and what are the elements of its orbit? * The great difficulty here encountered was, the uncertainty relative to the precise ellipse described by Uranus, because according to that orbit's variation the stranger's supposed influence must be varied. It is requisite to form the expressions of the perturbations due to the hypothetical body, in functions of its mass and of the unknown elements of the ellipse which it describes: these perturbations must be introduced into the co-ordinates of Uranus, computed by means of the unknown elements of this planet's orbital ellipse. Equating the co-ordinates thus obtained to those observed, the elements of the ellipse described by both planets must be regarded as unknown in the equations of condition which result therefrom. Then, rigorously eliminating the orbital elements of Uranus, we obtain the relations between the stranger's mass, the excentricity of its orbital ellipse, and its mean longitude at the commencement. These new relations determine the expressions of the orbit's eccentricity, and the longitude of the perihelion, in functions of the mass, and of the longitude of the epoch. Finally, only the mass of the planet and the mean longitude at the original time will remain arbitrary; and these must be selected so as to suit the rest. Such are hard conditions, my masters: but Le Verrier proceeds under them to investigate such an orbit, assuming a mean distance suggested by Bode's Law, and concludes by pronouncing as the most probable result of his investigation, that the true heliocentric longitude of the disturber will be about 325° for the 1st of January, 1847—confidently affirming, moreover, that an error of 10° in this place is not probable.

Never was a bolder paper than this thrown out to public scrutiny; and it was deemed conclusive, although it gave but a single element, without affording results respecting the stranger's mass, or the form of its orbit. But in the *Comptes Rendus* for 31st August, 1846, M. Le Verrier communicated a third paper—and a wonderful production it is—in which he proceeded to fix more exactly the place and distance of the yet unseen planet, the size being pronounced on a pure hypothesis respecting its density. He now distinctly described it as a body many times the magnitude of the Earth, and not much less than Saturn, taking more than two centuries to revolve about the Sun, at a distance 32 times greater than that of the Earth. To obtain the quantities to substantiate these views, he grouped all the observations into 33 equations, explaining the peculiar method by which he derives the values of unknown quantities from them: and the following constitute the elements which he thus acquired for the 1st of January, 1847—

* Perhaps the reader will here like the *ipsissima verba* of M. Le Verrier, since they are as remarkable as terse:—"Nous sommes ainsi conduits à nous poser la question suivante: *Est-il possible que les inégalités d'Uranus soient dues à l'action d'une planète, située dans l'écliptique, à une distance moyenne double de celle d'Uranus? Et s'il en est ainsi, où est actuellement situé cette planète? Quelle est sa masse? Quels sont les éléments de l'orbite qu'elle parcourt?*"

Mean Longitude	318° 47′
Perihelion	284° 45′
Semi-axis major	36·154
Excentricity	0·10761
Periodic time, sidereal years	217·387
Mass (Sun as unity) $\frac{1}{9322}$	0·0001075
True heliocentric longitude	326° 32′
Radius-Vector	33·06

Within one month after the Philosopher had thus minutely fixed and published beforehand the place of this mysterious body, defining the limits between which its *locus* must be sought, it was actually *bagged!* and the news clapped a stopper on the grave demurs of sceptics, who admitted the plausibility of the conjecture, but had denied its practical application. In a letter received at Berlin on the 23rd of September, M. Le Verrier urged Dr. Galle, of the Observatory at that place, to sweep sharply for the new star, which he expected would be recognised by its disc: why this was not requested of the astronomers at Paris is not in evidence. That very evening Galle repaired to his post, and on comparing the aspect of the examined region of the heavens with Bremicker's excellent map, Hora XXI., he very soon found a star of about the 8th magnitude, nearly in the place pointed out by Le Verrier, which did not exist in the map. There was little or no doubt that this was the new planet; it was compared three times that night with a known fixed star, and an orbital motion was suspected; this was soon confirmed, and the observations of the two following days showed that its march was in the direction of, and nearly equal to, the prediction. Le Verrier's presumed diameter was 3″·3; and it may now be considered as established at 2″·8. But, on the very first mensuration, with illuminated wires and a power of 320, the diameter was found to be 2″·9 by Professor Encke, and 2″·7 by Dr. Galle: when the field was enlightened the same observers found the diameter to be 3″·2 and 2″·2 respectively,—but the later observations were made under unfavourable atmospheric conditions. This coincidence between *measure* and *estimate* is truly admirable, and shows the wonderful sagacity with which the existing data had been made use of. After an examination of three days' observations, Encke pronounced that "the place of the planet agrees *within one degree*;" which correspondence between theoretic computation and actual observation must have been fully gratifying to Le Verrier, who, in concluding his Memoir, observes:—"The error in my computation will be considered very trifling, when we reflect on the smallness of the perturbations from which the place of the planet had been inferred. This success allows of a hope, that after 30 or 40 years' observations of this new body, it may in turn be employed as a means of detecting the next which follows it in the order of distance from the Sun. Thus, in the sequel, we shall unfortunately arrive at planets invisible on account of their immense distance from the Sun, but the orbits of which will be correctly traced, in the course of centuries, by means of the theory of secular inequalities."

Thus, then, by a profound computation based on very slight data, the closet mathematician has been armed with more than magician's power, and not only pointed out the place in which a new planet must be found, but also limited the space in which to search for it, weighed its mass, determined its diameter, figured its orbital circuit, and numbered the years of its revolution around

the central luminary! The confident temerity of the prediction was astounding, and staggered even the incredulous, for the most sanguine hope could hardly have expected so full a confirmation. "I cannot attempt to convey to you," said Mr. Airy, "the impression which was made on me by the author's undoubting confidence in the general truth of his theory, by the calmness and clearness with which he limited the field of observation, and by the firmness with which he proclaimed to observing astronomers, *Look in the place which I have indicated, and you will see the planet well.*" And this wondrous and successful exertion of the powers of abstruse research was received with a burst of rapturous applause throughout the intellectual world; and numerous are the well-deserved honours showered on Le Verrier. His own Sovereign ordered his bust to be placed in the College of St. Lô, and decorated him with the cordon of the Legion of Honour, and he was installed into the chair vacated by Count Cassini at the Institute; the King of Denmark enrolled him among the Knights of the Danebrog; and the Emperor of Russia conferred on him, by rescript, the second class of the Order of St. Stanislaus. In England he has been unanimously elected into the Royal and the Royal Astronomical Societies: and the former body awarded him the Copley medal for 1846, their highest honour. Of the Astronomical Society's medal we shall presently speak.

Since this singularly grand discovery, the stranger has been frequently scrutinized, both in our own country, in various parts of the continent, and in North America. Thus the known boundaries of our planetary system have been at once nearly trebled, and a body added to it which though a mere clod, and utterly useless in a worldly point of view, is a gem of the first order in confirming the truths of the Newtonian doctrines. At present it has an apparently retrograde motion, amounting to two or three seconds of time daily; but its actual mean hourly motion in orbit must be about 12,000 miles, which is not half the movement of the once-designated *sluggish* Saturn, and is something less than one-sixth of our own rate of going. With a diameter of 43,000 miles, and a bulk nearly 200 times that of the Earth, it has a periodic revolution round the Sun of about a couple of centuries. To the slightly aided eye it appears as a star of the 8th magnitude, but a disc is raised under comparatively easy telescopic power. Our excellent friend Mr. Lassell, of Liverpool, has, moreover, viewed it with the largest *equatoreal* instrument in existence, viz. a Newtonian reflecting telescope of his own construction, with an aperture of 24 inches, and a focal length of 20 feet. With this admirable machine, under powers varying from 316 to 567, he sees something crossing the disc, and also a probable satellite. The atmospheric conditions have not been favourable, but the streak is seen in the same direction, using two different mirrors, and by several observers. His own words are—"On the 3rd of October, at about 8¾ hours, I observed the planet to have apparently a very obliquely-situated ring, the major axis being seven or eight times the length of the minor, and having a direction nearly at right angles to a parallel of declination. At the distance of about three diameters of the disc of the planet northwards, and not far from the plane of the ring, but a little following, there was situate a minute star, having every appearance of a satellite. I observed the planet again about two hours later, and noticed the same appearance. * * * * With regard to the existence of the ring, I am not able absolutely to declare it, but I received so many impressions of it, always in the same form and direction, and with all the different magnifying powers, that I feel a very strong persuasion that nothing but a purer state of atmosphere is necessary to enable me to verify the discovery. Of the existence of a star having every aspect of a satellite, there is not the shadow of a doubt. Afterwards I turned the telescope to the Georgium Sidus

Uranus), and remarked that the brightest two of his satellites were both obviously brighter than this small star accompanying Le Verrier's planet." Since this communication was made by Mr. Lassell, the existence of the ring has been confirmed at Cambridge; but there are several points which await the further scrutiny of science.*

Meantime we will submit an extract from the official Report which Professor Challis made to the Syndicate of that University, on the 22nd of March, 1847:—

"On Jan. 12, I had for the first time a distinct impression that the planet was surrounded by a ring. The appearance noticed was such as would be presented by a ring like that of Saturn, situated with its plane very oblique to the direction of vision. I felt convinced that the observed elongation could not be attributed to atmospheric refraction, or to any irregular action on the pencils of light, because when the object was seen most steadily I distinctly perceived a *symmetrical* form. My assistant Mr. Morgan, being requested to pay particular attention to the appearance of the Planet, gave the same direction of the axis of elongation as that in which it appeared to me. I saw the ring again on the evening of Jan. 14. In my note book I remark, 'The ring is very apparent with a power of 215, in a field considerably illumined by lamp-light. Its brightness seemed equal to that of the Planet itself.' On that evening Mr. Morgan, at my request, made a drawing of the form, which on comparison coincided very closely with a drawing made independently by myself. The ratio of the diameter of the ring to that of the Planet, as measured from the drawings, is about that of 3 to 2. The angle made by the axis of the ring with a parallel of declination, in the south-preceding or north-following quarter, I estimated at 60°. By a measurement taken with the position circle on Jan. 15, under very unfavourable circumstances, this angle was found to be 65°. I am unable to account entirely for my not having noticed the ring at an earlier period of the observations."

All this is most truly wonderful! In a region so awfully remote as nearly three thousand millions of miles from the grand central luminary, this extraordinary planet can receive but $\frac{1}{900}$th part of the light and heat which we enjoy. The feeble effects of the sun there would almost point out that we have now reached the very utmost bounds of his influence: but should there still be another exterior planet, which Neptune's backings and fillings may yet indicate, it will probably be nearly six thousand millions of miles distant, where the Cimmerian glimmer can only amount to $\frac{1}{3600}$th of the earth's light. This interferes with that dogmatic fitness of things which of yore it was pronounced by Sages, when Jupiter was shown to have four moons to compensate his far removal from the fountain of light, and Saturn, being then considered outside all, was allowed both a ring and satellites for the same object.

Such was the discovery, and such its conditions, as it met the public ear; and never was greater homage paid to cultivated thought than in the gratulations with which that public greeted the event. But as if the whole phenomenon was to be a startling affair, instead of the discovery

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* The case of a ring must probably remain undecided for a few years, until the planet occupies a better situation in the ecliptic, by rising in declination to a good-working altitude: but as we happen to know personally that its existence is now doubted by Mr. Lassell himself, 'tis as well to say so. The existence of the satellite was speedily confirmed by Otto Struve, in Russia, and G. P. Bond, in America.

being viewed with unmingled admiration, a new incident shook the opinions of men, and awakened considerable personality in certain quarters. It must be acknowledged that, considering national bias and excitement, something may-be pleaded in extenuation. In brief, it appears that, though the French geometer was so justly taking his triumphant lead, an English one had been some time steering the same course with wet canvas close on his weather-beam; and who, had the look-outs been at their posts, with the same charts on board, would assuredly have been far a-head. Indeed, though without any intention of taking the shine out of the chace, it might be seen, by all who had sailed for it, that there must inevitably be a close shave. This certainly took great numbers flat aback: the world at large were aware of the merit of Le Verrier, but, until the unexpected announcement of Messrs. Airy and Challis, they knew nothing of the fact with which some few—ourselves among the number—were partially acquainted, viz., that a young Cambridge mathematician, hight J. C. Adams, as aforesaid, had been already led, by his own spontaneous thought and independent researches, not only to conclude that a planetary body more distant than Uranus actually existed, but also most skilfully to point out its habitat and features. Now as we conceive this point to be one of some historical and scientific moment, and as it has already been the cause of much difference of opinion, accompanied with some ebullition of small feeling, we will submit the leading facts of the case. We trust that a fair statement will put matters to rights,—for it were lamentable indeed that a new planet should prove an apple of discord, and disturb the harmony of transcendental astronomers: even the steps recommended by justice can be but ill executed by petulance.

We will, therefore, proceed to examine the evidence, both formal and objective, begging the the courteous reader to recognise the hackneyed adage—*audi alteram partem*—the while. Some of the points advanced may bear an inconsistent aspect; but all who bring their best judgment to bear on the subject, will easily discern how to allow for lee-way and thereby correct the reckoning, so as to reduce it to the criterion of Truth.

Under this impression we shall, at the most knotty turns of the case, hand in the special test of official documents. Now it appears from the Report of the Astronomical Society for November, 1846, as well as that made by Professor Challis to the Syndicate of Cambridge on the 12th of December following, that Mr. Adams had long formed the resolution of trying, by calculation, to account for the anomalies in the motion of Uranus: "He showed me," says Mr. Challis, "a memorandum made in 1841, recording his intention of attempting to solve this problem as soon as he had taken his degree of B.A. Accordingly, after graduating in January, 1843, with the honour of Senior Wrangler, he returned to the encounter, and obtained an approximate solution by supposing the disturbing body to move in a circle at twice the distance of Uranus from the Sun. The result so far satisfied the apparent anomalies in the motion of Uranus, as to induce him to enter upon an exact solution." For this purpose he required a set of reduced observations, and applied to obtain them from Greenwich,* through the intervention of Mr. Challis; and this was the first distinct intimation to the Astronomer-Royal:—

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* We should here state, that the first clear exhibition of the theory of Uranus was certainly made by the routine operations at the Cambridge Observatory, and the beautiful reductions there tabulated were eminently useful in all stages of Neptune's discovery.

"Cambridge Observatory, Feb. 13. 1844.

"A young friend of mine, Mr. Adams, of St. John's College, is working at the theory of Uranus, and is desirous of obtaining errors of the tabular geocentric longitudes of this planet, when near opposition, in the years 1818—1826, with the factors for reducing them to errors of heliocentric longitude. Are your reductions of the planetary observations so far advanced that you could furnish these data? and is the request one which you have any objection to comply with? If Mr. Adams may be favoured in this respect, he is further desirous of knowing, whether in the calculation of the tabular errors any alterations have been made in Bouvard's Tables of Uranus besides that of Jupiter's mass."

To this application, Mr. Airy immediately returned this reply:—

"Royal Observatory, Greenwich, 1844, Feb. 15.

"I send all the results of the observations of Uranus made with both instruments (that is, the heliocentric errors of Uranus in longitude and latitude from 1754 to 1830, for all those days on which there were observations, both of right ascension and of polar distance). No alteration is made in Bouvard's Tables of Uranus, except increasing the two equations which depend on Jupiter by $\frac{1}{50}$ part. As constants have been added (in the printed Tables) to make the equations positive, and as $\frac{1}{50}$ part of the numbers in the Tables has been added, $\frac{1}{50}$ part of the constants has been subtracted from the final results."

Dates now begin to be of paramount interest in the story, since a very discreditable rumour obtained among the Demotæ of science, to which we must presently allude, on account of its notoriety. The next letter which appears, shows that Mr. Adams derived advantage from the communication; it is from Mr. Challis to the Astronomer-Royal:—

"Cambridge Observatory, Sept. 22, 1845.

"My friend, Mr. Adams (who will probably deliver this note to you), has completed his calculations respecting the perturbation of the orbit of Uranus by a supposed ulterior planet, and has arrived at results which he would be glad to communicate to you personally, if you could spare him a few moments of your valuable time. His calculations are founded on the observations you were so good as to furnish him with some time ago; and from his character as a mathematician, and his practice in calculation, I should consider the deductions from his premises to be made in a trustworthy manner. If he should not have the good fortune to see you at Greenwich, he hopes to be allowed to write to you on this subject."

To this Mr. Airy appends a remark: "On the day on which this letter was dated, I was present at a meeting of the French Institute." This incidental observation, slight as it is, had raised a bubble in the minds of some of the magnates of the periodical press, and several of their followers. A sturdy assailant took the field in the Mechanics' Magazine, and, unprovided with either proof or probability, trumpeted the delinquency of the Astronomer-Royal to the world: how that he, sojourning in Paris, did then and there most imprudently, as well as naughtily, let the cat out of the bag, supplied Le Verrier with Adams's work, and informed the wondering Frenchmen all about the new planet. Yet this Seer cannot have had the slightest basis for so barefaced an assertion; for from the incontrovertible internal evidence of the Report read to the Astronomical Society, and which we are quoting, the Astronomer-Royal must be acquitted of the silly but foul

charge by every pure-minded investigator. On receiving a copy of Le Verrier's Memoir, on the 23rd or 24th of June, of the following year, he thus returned his acknowledgments:—

"Royal Observatory, Greenwich, 1846, June 26.

"I have read, with very great interest, the account of your investigations on the probable place of a planet disturbing the motions of Uranus, which is contained in the *Compte Rendu de l'Académie* of June 1; and I now beg leave to trouble you with the following question. It appears, from all the later observations of Uranus made at Greenwich (which are most completely reduced in the Greenwich observations of each year, so as to exhibit the effect of an error either in the tabular heliocentric longitude, or the tabular radius vector), that the tabular radius vector is considerably too small. And I wish to inquire of you whether this would be a consequence of the disturbance produced by an exterior planet, now in the position you have indicated?

"I imagine it would not be so; because the principal term of the inequality would probably be analogous to the Moon's variation, or would depend on sin. $2\ (v-v')$; and in that case the perturbation in radius vector would have the sign — for the present relative position of the planet and Uranus. But this analogy is worth little, until it is supported by proper symbolical computations."

Now here there is not the most distant allusion to Mr. Adams, which must have been the case had the writer committed himself at Paris, after the manner so deliberately alleged.

Most of Adams's friends were staggered by the boldness of his problem, as announced by so young a mathematician: and though he showed that his hypothetical body would satisfy all the anomalies in the most trustworthy observations of Uranus, still, under what they deemed a justifiable scepticism, they lost the moment for victory. Had there been hope and confidence Le Verrier and Adams must have changed places; but while the former was brought out in full daylight, the latter was shrouded in secrecy—or at least to what in effect almost amounted to secrecy. Though the basis was sound, there was not sufficient faith; so that even this, the first instance of a solution of the abstruse and difficult analytical investigation of the inverse problem of perturbations,* was not made public. It was unfortunate that it appeared to the Plumian Professor as "so novel a thing to undertake observations in reliance upon merely theoretical deductions, and that, while much labour was certain, success appeared very doubtful," that he neither engaged in the pursuit himself, nor afforded to others the means of doing so. Under a similar misgiving, the Astronomer-Royal says, that when he found Le Verrier's place for a disturbing planet was the same, to one degree, as that given by Mr. Adams's calculations, which he had perused seven months earlier, he began to look to it. "To this time," he says, "I had considered that there was still room for doubt of the accuracy of Mr. Adams's investigations; for I think that the results of algebraic and numerical computations, so long and so complicated as those of an inverse problem of perturbations, are liable to many risks of error in the

* The inverse problem of perturbations, is that in which the computations may be made from apparently anomalous motions in the body under influence, and not from the known attractions of the body influencing: in other words, from known disturbances of a planet in known positions, to find the place of the disturbing body at a given time. Here, as the reason necessarily bears from the effect to the cause, and not from the cause to the effect, for that was unknown, the problem was one of extreme difficulty, and heretofore—as far as we know—untried.

details of the process. I know that there are important numerical errors in the *Mécanique Céleste* of La Place; in the *Théorie de la Lune* of Plana; above all, in Bouvard's first *Tables of Jupiter and Saturn;* and, to express it in a word, I have always considered the correctness of a distant mathematical result to be a subject rather of moral than of mathematical evidence. But I now felt no doubt of the accuracy of both calculations, as applied to the perturbation in longitude. I was, however, still desirous, as before, of learning whether the perturbation in radius vector was fully explained."

The latter remark brings us upon another point in this curious and eventful bit of history. When Mr. Adams made his first statement, Mr. Airy most properly requested to know, "whether the assumed pertubation will explain the error of the radius vector of Uranus?" To this inquiry, from some unexplained cause, no immediate answer was returned: but on asking Le Verrier the same question, he received a ready and precise reply,—that the radius vector was rectified by itself without its having been taken into direct consideration; and he added, "*Excusez moi, Monsieur, d'insister sur ce point. C'est une suite du désir que j'ai d'obtenir votre suffrage.*" We can readily allow for the cautious feeling which made this question so strongly insisted upon, as a crucial instance of the actual strength of the discovery, and it ought to have been answered in some way or other: but unfortunately no reply was given. But this should not have made an obstruction, especially as Adams had eliminated all the errors of longitude, which was his principal object; and it seems that he actually employed a method of calculation which required him to compute the co-efficients of the expression for error of radius vector, *before* computing the co-efficients of the expression for error of longitude. It is, therefore, to be regretted that this co-ordinate should have impeded the Cambridge correspondence by giving, however unintentionally, the appearance of a slight to the referee. Nor is this the only vexation. Adams's definite results ought to have been printed, at least, immediately on the appearance of Le Verrier's first announcement: and Mr. Airy has stated that, had he received an affirmative reply to his question, he would at once have exerted all the influence he might possess, to procure the publication of Mr. Adams's theory—directly or indirectly —through Professor Challis.

The plot was now thickening. At a meeting of the Board of Visitors of the Royal Observatory at Greenwich, at which we were present, the Astronomer-Royal alluded to the impending discovery of a new planet, since there was a singular accordance between the investigations of Adams and Le Verrier, who, although starting on the same principle, had arrived by independent trains of reasoning at the same result. From this remark—and here we speak advisedly, though not in accordance with M. Arago's argument—originated the eloquent expression of Sir John Herschel to the British Association, at Southampton, on the 10th of September. Having observed that the last year had given another new planet (*Astrea*) to our system, he added,—"It has done more: it has given us the probable prospect of another. We see it as Columbus saw America from the shores of Spain. Its movements have been felt, trembling along the far-reaching line of our analysis, with a certainty hardly inferior to that of ocular demonstration." And the same discussion led Professor Challis to contemplate a search for the suspected disturber,—a search not before thought of, seeing that he had already full work of his own to labour through.

The Astronomer-Royal transmitted to Cambridge suggestions for the examination of a region

of the heavens 30° long, in the direction of the ecliptic, and 10° broad, having the theoretical locus of the planet at its centre: and at the same time he made a liberal offer of assistance, even at his own cost, the which, to our surprise, was not accepted. A modification of the suggested plan was adopted, regular approaches were formed, and 3,150 positions of stars were recorded; but it so happened that this was like sweeping a large Turkey carpet in quest of a missing diamond, which might have been detected by its inherent brilliance on the spot where it was dropped; and though this course was adopted to prevent ultimate disappointment, yet, as we presume, a careful scrutiny with the large and powerful telescope employed, must have produced the planet in the early part of August. The opportunity of first announcing its optical discovery was lost. Mr. Adams had found the mass to be about three times that of Uranus, and had thence inferred that the brightness would not be below that of a star of the 9th magnitude; but his consequent request that the planet might be sought for by its physical aspect was neglected. This is matter of regret, since, from the surpassing interest of the question, it ought to have been fished for nine months before, namely, in October, 1845, when both the prediction and the detection would infallibly, and without competition, have fallen to Cambridge; and England would have enjoyed an incontestable right to a sort of astronomical feat which, great as she is, she is most in want of. The following are the elements on which the scrutiny was eventually conducted:—

	Hypothesis I. $\left(\frac{a}{a^1}=0{\cdot}5\right)$	Hypothesis II. $\left(\frac{a}{a^1}=0{\cdot}515\right)$
Mean Longitude of Planet, 1st Oct., 1846	325° 8′	323° 2′
Longitude of Perihelion	315 57	299 11
Eccentricity	0·16103	0·12062
Mass (that of Sun being 1)	0·00016563	0·00015003

Such being the conditions of the case, we must proceed to consider them, and we trust at least to bring impartiality to bear. According to the Astronomer-Royal's incontestable evidence, no doubt can be entertained of Adams's being *de facto* the first to predict the existence and locus of a new planet. Such a body was *à priori* probable; and the skilful geometer showed, by giving all the possible elements (*node and inclination out of the question*) and the place at a given time,—that such a body would satisfactorily account for the errors observed in the motions of Uranus. Why he did not explain Mr. Airy's query about the radius vector is not in evidence; but the errors of that condition are readily deducible from both the above-cited hypotheses. It is also now quite clear that the Cambridge astronomer had actually got sights of the planet on the 4th and the 12th of August, seven weeks before Dr. Galle's discovery of it; but he assuredly was not aware of it; for he says—"After four days of observing, the planet was in my grasp, *if I had only examined or mapped the observations*,"—"my observations would have shown me the planet in the early part of August, *if I had only discussed them.*" "I lost the opportunity of announcing the discovery, *by deferring the discussion of the observations*, being much occupied with the reductions of comet observations, and *little suspecting that the indications of theory were accurate enough to give a chance of discovery in so short a time.*" That the observer was not really aware of the planet's having been

caught, and that he did not even expect it was, is evident from the following letter, written by him to the Astronomer-Royal:—

"Cambridge Observatory, Sept. 2, 1846.

"I have lost no opportunity of searching for the planet; and, the nights having been generally pretty good, I have taken a considerable number of observations; but I get over the ground very slowly, thinking it right to include all stars to 10-11 magnitude; and I find that, to scrutinise thoroughly in this way the proposed portion of the heavens, will require many more observations than I can take this year."

And he further declares, that, on receiving tidings of the planet's discovery at Berlin, he was so much impressed with the sagacity and clearness of M. Le Verrier's limitations of the field of observation, that he instantly changed his plan of observing, and noted the planet, as an object having a visible disc, *on the evening of the same day!* Indeed the Professor's own statements open the door of controversy, both as to the actual discovery and the precedence of publication: "A comparison," he says, "of the observations of July 30 and August 12, *would,* according to the principles of search which I employed, have *shown me the planet.* I did not make the comparison of it till *after the detection of it at Berlin,* partly because I had an impression that a much more extensive search was required *to give any probability of discovery,* and partly from the press of other occupations." And though he noted the planet on the 29th of September, he merely directed his assistant to write against that *star,* it "*seems to have a disc,*"—so that uncertainty reigned till the Berlin news arrived on the 1st of October, when "all was light." The Cambridge measure was both regular and sure, but was deficient in faith and alacrity: nor can we ponder on the grand event without a strong sensation of regret that the illustrious rivals were not practical astronomers as well as great mathematicians, so that, instead of beating the bush for aid, they could have observed for themselves.

As M. Le Verrier's Memoirs were under publication, it became necessary to print also Mr. Adams's calculations and formulæ; but, since an unavoidable delay must occur in the medium which he resorted to, Lieutenant Stratford, the able Superintendent of the Nautical Almanac, came to his relief by a timely offer of printing the document as a Supplement for the Ephemeris of 1851, but with a view of circulating a number of copies of it forthwith. This will be best told in the Lieutenant's official notice prefixed to the paper:

"This paper was communicated by the Author to the Royal Astronomical Society, and was read to that body at their ordinary meeting on November 13, 1846. The press of the Society being engaged on an extensive paper on the longitude of Valentia by the Astronomer-Royal, and it being deemed of national importance that Mr. Adams's paper should be submitted to the world without loss of time, application was made to Capt. W. H. Smyth, R.N., President, and to the Rev. R. Sheepshanks, Secretary of the Society, who, with their usual promptitude and zeal, granted permission for the immediate printing and publishing of the paper by the Nautical Almanac Office; and it is under these circumstances that the investigations of Mr. Adams first appear as an extract from the Appendix to the Nautical Almanac for 1851.

"W. S. Stratford, Superintendent of the Nautical Almanac.

"Nautical Almanac Office, 3, Verulam Buildings, Gray's Inn, London,
"December 31st, 1846."

The publication of his method showed that Mr. Adams had not arrived at his conclusions by rough estimation, or graphical leaps; and that while his advance was cautious, his steps were masterly and refined. But though he was thus raised in the public opinion, it did not shake Le Verrier's claim a whit. The completeness of that thorough geometer's work, added to the orderly decision with which he conjured Astronomers to mark down the quarry, excite our warmest admiration. No petty jealousies ought to defile this feeling. By all the rules of fair adjudication, the noble prize is his; nor has anybody tendered it in more appropriate and truly liberal terms than his excellent competitor. "I mention these dates," says Mr. Adams, "merely to show that my results were arrived at independently, and previously to the publication of those of M. Le Verrier, and not with the intention of interfering with his just claim to the honour of the discovery; for there is no doubt that his researches were first published to the world, and led to the actual discovery of the planet by Dr. Galle; so that the facts stated above cannot detract in the slightest degree from the credit due to M. Le Verrier." This is somewhat of a contrast to the virulent conduct of certain French journalists, and the frothy excitement of others on record, who cannot plead the proverbial hastiness of youth in extenuation. Even the *liberal* Mons. Arago, albeit delighted at our system being enriched with a new constituent, would not only have ignored and swamped the Cambridge efforts, but most precipitately and harshly enounced—that Mr. Adams is not entitled to the *slightest allusion* in the history of the discovery!" (*Comptes Rendus*, 19 *Oct.*, 1846.)* But M. Biot, on the contrary, giving full credit to our countryman, thus speaks:—"Je ne parle pas ici d'après ce sentiment d'égoïsme géographique, appelé si improprement du patriotisme. Les esprits voués à la culture des sciences ont, à mes yeux, une commune patrie intellectuelle, qui embrasse tous les degrés d'élévation du pôle." This is the language of true philosophy: it is certainly no disparagement of Adams's claims to distinction, to say that the glory of this discovery belongs to Le Verrier; nor does a full acknowledgment of the merits of the one detract from those of the other.

Still the sound and brilliant claim of Mr. Adams has been rather tarnished than otherwise by the well-intentioned but injudicious clamour of over-zealous friends, some of whom would fain throw Le Verrier overboard. Of this a remarkable instance occurred at the Tercentenary dinner of Trinity College, on 22nd December, 1846. On this occasion the Chairman declared, in Arago-nian assumption, to the assembled guests—"If they needed anything to remind them of that (*the necessity of Colleges maintaining a community of interest*), they would find it in the reflection that the great discovery in Astronomy by which this age would be known, was due to one of their friends on the other side of the wall." † (*Loud cheers.*) This, like the food and the wine, was willingly swallowed; but we are compelled, by the facts before us, to consider it as a decided over-statement, or rather a *post-prandium* ebullition. Still, springing as it did from the University banquet, the news quickly flew throughout the land on the wings of the press; yet, had not Le Verrier's announcement of the planet's

* Arago acknowledges that Adams investigated the problem with ability, and solved it, but denies that his labours contributed to the discovery: "Mais, je me hâte de le dire, Mr. Adams ne publia rien, et son travail, très-bien fait d'ailleurs, ne servait en aucune façon à faire découvrir l'astre inconnu." Nay more, in his recent list of astronomical events, published after he had had sufficient time to cool and reflect, he names Le Verrier and Galle, but omits all mention of our countryman.

† To those who are unacquainted with the topography of Cambridge, it may be told that a high wall divide the Colleges of Trinity and St. John's.

locus appeared, it is not improbable that no large telescope would have been directed to the heavens in search of it. To be sure Mr. Adams *might* have still worked at his theory—he *might* have insisted on its publication in its then state—he *might* have published it himself—or he *might* have communicated his elements to the Astronomical Society, &c., &c.; but this is quite irrelevant, for none of these things were done, and no telescope was expressly turned to the sky till the publication of Le Verrier's results *forced* it to be done. We cannot therefore understand—whatever claim may exist on other scores—how the discovery of the planet was "due" to Mr. Adams's researches. Assuredly the contrary is the case. Galle looked for it and found it by Le Verrier's instructions solely. No one can show that he looked for it purely and simply by Adams's, nor was it begun to be fished for here before we had a knowledge of Le Verrier's conclusions. Q. E. D.

A very secondary kind of *friend* took up the cudgels, and, in a 'presto' book about Neptune, belaboured the Royal Society with an explosive discharge of turgid bombast. Forgetting what Shakspeare decreed, namely, that "an two men ride a horse, one must ride behind," he thus levels the *brutum fulmen*:—"The most venerable Society through its history in Europe—which, in its place as the assumed and sustained head and fosterer of British endeavour, could—in that hour, too, of difficulty, it might even be of despondency, because of misfortune and menaced injustice—overlook Mr. Adams's ever-memorable efforts, and, without one word of apology or explanation—without inquiry even—rush past to his rival and lay its honours at his feet! I understand the vindictiveness of Arago—I can understand on his part a vehemence prepared even to crush—worthily or otherwise—alike truth and opposition; for *la Grande Nation* has never been in high repute for tenderness towards men who would share its glory, or of toleration for foreign claims. But that the body in England whose duty—if not its foremost one, certainly with lower sanctions than none—has been understood by the State to be protective of the rights of its meritorious countrymen—that this body, composed of the *élite* of British men of science, and of British noblemen and gentlemen, should have bowed itself before Gallic pretension, and purchased some hollow compliments about liberality and freedom from prejudice, by the sacrifice of the then obscure graduate of Cambridge; yes, this almost inclines one to hope that the name of Adams may never illustrate its roll!"

This trash would be libellous, but that—from its utter want of truth—it never could at all injure the Royal Society. We can here speak point-blank, having been upon the Council in 1846, and also chairman of the committee which recommended Le Verrier to the Council's notice, for his investigations relative to the disturbances of Uranus, by which he proved the existence and predicted the place of the constructive planet—a prediction, confirmed as it was, to be considered among the proudest triumphs of modern analysis as applied to the Newtonian theory of gravitation. There was one medal to award, and the opinion of the true Britons in conclave was, that they assigned it to a meritorious and amazing stretch of science and intellect: and moreover the step was taken before a single publication by Mr. Adams, or any of his friends on his behalf, had been made. But on the 28th of October, 1847, the Astronomical Committee of the Royal Society, at a sitting which we also attended, recommended to the Council to award the Copley medal to that gentleman for his inquiries relative to the anomalies of Uranus, and for his being the first who applied the theory of perturbations inversely, to discover, from the disturbances of the perturbed body, the place and elements of motion of the perturbing body. But as Sir John Herschel had been nominated for that year's

medal, in recognition of his splendid gratuitous labours in South Africa, Mr. Adams received the one for 1848, and in June 1849 he was unanimously elected a Fellow of the Royal Society. So much for the Glasgow Professor of Astronomy!

Under such unquestionable facts, the debate of absolute priority is one of grave import, and must ultimately depend upon what may be deemed the publication of the problem in question. A large and rather influential party adhere to a somewhat lop-sided decision of the deplorable contest between Leibnitz and Newton, and cite all sorts of unilateral incidents, which may be deemed rather exceptions than rules: but in the present "enlightened" day, one would no more think of reverting to logogriphes than of sending a parcel by packhorse to Bristol. The custom of Galileo and Co. would be about as antiquated as now keel-hauling a sailor for rapping out an oath would be. We give Mr. Adams the full benefit of that axiom in law, which decides that such evidence as a jury may have by their private knowledge of facts, has as much right to sway their judgment as the written or parole evidence which is delivered in court: but we also recollect that in law, ever since the dispute between Euphorbus and Menelaus, it is contested whether he that first wounds a beast that is classed among the *feræ naturæ*, or he that kills it, was to bear off the spoil and quarry. In the case before us, it seems that Adams shot at the stranger, but Le Verrier brought him down. It were better, therefore, that they remain Arcades-ambo through successive ages than attempt a division of interests: joined, as Herschel says, by Genius and Destiny, they must pass to posterity as the Siamese twins of Science. Lord Mansfield, however, if we may judge by his important decision in Dollond's celebrated achromatic-telescope trial, would have declared for the Frenchman: when it was pleaded as an objection to Dollond's patent, that Dr. Hall had made the same discovery many years before, his Lordship held that, as the public were not acquainted with the fact, Mr. Dollond must be regarded as the true inventor: he was not only a discoverer of it as well as Dr. Hall, but, being the *first publisher*, was fully entitled to all the benefit. So Waring also states—"That person is the inventor who first *publishes* his discovery;" but if, from diffidence, design, or carelessness, he does not communicate it, then such *lâcheté* establishes a second-comer in equal rights.

These considerations involve a sort of necessity to offer a few words on the recent conduct of the Royal Astronomical Society also, in regard to the non-award of their medal in the present unprecedented instance, an instance such as can seldom occur, and probably never will recur again. It is well-known that it is in the power of the Council of this meritorious Society to confer one gold medal annually upon the most important astronomical discovery of the year. But to prevent the award of medals to unimportant communications, a bye-law—and perhaps a wise precaution—requires that there shall be a majority of votes of three to one in order to give the prize. On this occasion there was a serious difficulty: two opinions prevailed—first, that a medal should be voted to M. Le Verrier alone; secondly, that, unless a medal were also given to Mr. Adams, a great injustice would be done. But the whole imbroglio will best appear, on citing the official minutes of their Anniversary Meeting: and thus they run—

"Friday, February 12, 1847.

"Capt. W. H. Smyth, R.N., President, in the Chair.

"Were it intended to describe the results of the century instead of the current year, the subject to which your Council now come would lose none of its prominent interest. The prediction of a new planet, on

grounds derived from calculation only—the fulfilment of that prediction—the attainment of the solution of the inverse problem of perturbation—mark the years 1845 and 1846 with an importance which belongs to no period except that of the announcement of the theory of gravitation and of the publication of the *Principia*.

* * * * * *

"The facts connected with this singularly splendid triumph of mind over matter have been much discussed, and are now fully published. The statement made to this Society by the Astronomer-Royal in November, the Memoirs of M. Le Verrier, the Memoir of Mr. Adams, and the statements made by Mr. Challis, and in various numbers of the *Comptes Rendus*, have put our Fellows in such possession of the absolute circumstances of the case as renders any detailed account of them unnecessary in this Report. It fortunately happens that there is no one disputed fact; but upon the construction of the facts, and upon the meaning of words, there are differences of opinion, at least as wide as those which have always existed upon the great question of the claims of Newton and Leibnitz to the invention of Fluxions.

"In one thing there is general agreement, namely, in giving both to Mons. Le Verrier and Mr. Adams the highest order of praise and admiration. As soon as they are compared, all manner of opinions are found to prevail as to their *relative* positions; but on the absolute character of the rank taken by the labours of both in the history of astronomical discovery there can be but one feeling.

"Under these circumstances, it will be matter of regret, but hardly, all things considered, one of astonishment, that your Council has not been able to give any verdict upon the disputed matters of opinion, nor to afford, to any conclusion, the sanction which would be considered as implied in the award of a medal to M. Le Verrier, to Mr. Adams, or to both. Such a tribute is not needed by either: and your Council distinctly request it may be understood, that, in making a statement of the circumstances under which they have failed to arrive at a decision, they are simply accounting to their constituents for their own conduct, and not intending to draw any conclusion upon the controverted opinions. Perhaps there is not one among them who does not, more or less, censure the collective body to which he belongs for not adopting a positive course; while, perhaps, there are very few indeed who could agree upon any one mode of proceeding. And it is by no means improbable, that the same general wish that something had been done, and the same disagreement as to what it should be, which has prevailed in the Council, would also prevail in the Society.

"By our bye-laws, only one medal can be given in any one year; but it is in the power of a General Meeting, at the proposal of the Council, to suspend or abrogate any bye-law. Again, by the same laws, all propositions for the award of medals must be made and seconded in November, and taken into consideration in January. That no possible view of the case might be precluded from discussion, the individual members of the Council, with whom every such proposition must originate, took care that the list of those nominated for the medal in November last should contain all the names which could by possibility come into question.

"The first point of discussion was, whether it would be expedient to recommend the General Meeting to suspend the existing bye-law, and to give the power of awarding more than one medal. This, it is very obvious, has in itself a question of expediency, totally independent of the particular circumstances under which the permission is sought: and a motion was made to the effect that such a course was not expedient. This motion was carried; and, as it may be presumed that the grounds on which it was brought forward are those on which it was carried, the Council think it right to state those grounds.

"In carefully guarding the decisions of the medal, by placing such awards wholly in the hands of the Council, and declaring that no medal shall be given by the Society at large, the latter body has made a

standing confession of the obvious truth that a large assembly of men, interested in astronomy in very different ways, does not form so proper a court for the decision of delicate questions of personal merit as a smaller body, chosen by themselves, out of all whose occupations will allow of their attendance, as a specified number of those who are best qualified to conduct the affairs of the Society. If we consider how many propositions it is open to any Fellow of the Society to make, and how few are made except through the Council, it would appear that the general feeling is, that the letter of the law respecting medals is only the expression of the spirit in which the Society desires that its business should be conducted.

"It was contended that this spirit of our laws would be violated, to the introduction of every disadvantage which those laws were intended to avoid, if a more than usually difficult question were submitted to the Society, of the very kind which the Society had peculiarly delegated to the Council, even in the ordinary and easier cases. Taking it for granted that the law was adopted for wise reasons, it was urged that it would be highly improper to force upon the general body the public discussion of the nicest question of relative merit which has arisen for more than a century; and it might reasonably be expected that the extremes of opinion found to exist in the Council might be taken as a low estimate of those to be looked for in a larger body. The motion founded upon this view of the case was carried.

"It being then decided that no recommendation to depart from the usual course should emanate from the Council, the question to whom the *one medal* should be awarded necessarily followed. The claim first considered was that of M. Le Verrier, whose name stood first on the list. This medal being, under the circumstances, an expression of opinion upon a matter likely to be long under discussion, or at least certain to be so interpreted both at home and abroad, it seems to have been thought by several that an award to M. Le Verrier, unaccompanied by another to Mr. Adams, would be drawing a greater distinction between the two than fairly represents the proper inference from facts, and would be an injustice to the latter. Accordingly, on a ballot being taken, it appeared that the majority in favour of the proposition was not sufficient to carry it, the bye-laws requiring that no medal should be awarded upon any majority of less than three to one. No award could therefore be made; and the Council can only conclude upon this matter, that the differences of opinion prevailing among the members render it impossible for them, as a body, to offer any statement upon the controverted points of the question.

"Perhaps it would not be improper to add, that, in a question in which a French and English claim are mixed, in a manner which requires a perfect absence of national feeling rightly to settle, it is not to be regretted that this Society should thus have been compelled, by the action of its own laws, to refer the decision to the astronomers who are of neither of the nations thus placed in opposition."

"The Report having been read, it was proposed by Mr. R. Taylor,—Seconded by Captain Sir John Ross: 'That the Report of the Council now read be received and adopted; and that it be printed and circulated in the usual manner.'

"Proposed in amendment by Mr. Babbage,—Seconded by Dr. Fitton: 'That this Meeting express their deep regret that the Council have not awarded the Society's medal to M. Le Verrier, for his publication of the greatest astronomical discovery of modern times.'

"This amendment was negatived.

"A second amendment was Proposed by Lieut. Raper, R.N.,—Seconded by Capt. Bethune, R.N.: 'That it is the opinion of the Meeting, that the unprecedented discovery of a new planet by theoretical researches, and the acknowledged title of M. Le Verrier to the honour of that discovery, demand for him some special mark of the approbation of this Society: that it be recommended to the new Council to convene a Special General Meeting of the Society, on as early a day as may be convenient, for the purpose of suspending

Articles 2, 3, and 4, of Section 16 of the bye-laws; and that the printing of the Report be deferred till the subject shall have been brought under the consideration of such Special General Meeting.'

"This amendment was also negatived.

"A third amendment was proposed by the Rev. R. Sheepshanks,—Seconded by Mr. Drach: 'That a Special General Meeting be called to consider the propriety of granting a medal to M. Le Verrier, for his researches respecting the planet exterior to Uranus; and also a medal to Mr. Adams for his researches on the same subject.'

"This amendment was also negatived.

"A fourth amendment was proposed by the Astronomer-Royal,—Seconded by Dr. Lee: 'That a Special General Meeting be called after the ordinary Meeting on March 12, to consider the following resolutions:—

"'That so much of the bye-law as relates to the number of medals which may be adjudged in any one year, the time of giving notice of the proposal for a medal, the time of adjudging the medal, and the time of presenting the medal, be suspended *pro hâc vice;*

"'That the Council be authorised to award two (or more) medals, if they shall deem it expedient to do so;

"'That the award of the Council be communicated to the Society, and that the medal or medals be presented at the ordinary Meeting of April 9th.'

"This amendment was carried.

"Proposed by Mr. De Morgan,—Seconded by the Rev. R. Sheepshanks: 'That this Meeting be adjourned to Saturday, Feb. 13th, at 2 o'clock.'"

Saturday, February 13, 1847.

"The Society met at 2 o'clock, according to adjournment, Captain W. H. Smyth, R.N., President, in the Chair.

"Proposed by Mr. De Morgan,—Seconded by Sir J. Ross: 'That the Report of the Council read yesterday be received and adopted, and that it be printed and circulated in the usual manner, with an account of the proceedings of the Meeting annexed.'

"Proposed in amendment by Mr. Babbage: 'That this Meeting do adjourn to a day to be named at the next General Meeting.'

"This amendment was not seconded. The original Motion was then put and carried.

"Proposed by Lieut. Raper,—Seconded by G. B. Airy, Esq.: 'That the thanks of the Meeting be given to the President for his conduct and temper in the Chair, during the continuance of this Meeting.'

"Carried unanimously."

The whole of these proceedings were conducted with sense and good feeling, although the combatants were at times rather warmly engaged; the chief interlocutors being severally under the full conviction of a righteous cause, in sifting the most striking application of the theory of gravitation made since it left the hands of Sir Isaac Newton. During the contest, all comparison between the respective merits of Adams and Le Verrier was so discouraged, that the few remarks of the kind which were uttered, fell dead. The bone of contention was, therefore, as to whether the bye-laws should be tampered with, or any interference be made in the Council's decision; that body having, as judges, acted to the best of their abilities, which is the only obligation they are morally placed under. When the Special General Meeting assembled to bring Mr. Airy's propositions under discussion, on March 12th, an amendment was proposed by Mr. De Morgan: "That, on taking all the circum-

stances into consideration, and particularly the existing differences of opinion on the subject, it is not expedient to propose to the Council to reconsider the subject of the medal." This amendment was carried; and the Meeting broke up with the highest respect for the rival candidates. All hands were fully persuaded that the orbit of a planet exterior to Uranus had been defined, and its locus predicted, by Adams, with sufficient correctness for picking it up, in November, 1845, had it been duly fished after; but that Le Verrier, on quite independent grounds, stepped in and triumphantly bore it off in September 1846. This is altogether a far more praiseworthy and gratifying specimen of competition between France and England, than some which might be cited.

We were about to close our lucubration, when we suddenly recollected that the style and title to be assigned to the new planet, have excited almost as much fermentation as its discovery had evulgated. Shakspeare, it is true, somewhat temerariously demands—"What's in a name?" but astronomers think, with Pythagoras, that "it requires much wisdom to give right names to things." Hence the mighty turmoil which still disturbs the atmosphere of science. While some wish the discoverer's name to be attached to any newly-detected celestial body, a still larger class are clamorous for retreating upon classical mythology, as neutral ground in unison with the existing order. Ophion, Gallia, Atlas, Chronos, Gravea, and Oceanus, were severally proposed and rejected. Janus—proposed by Dr. Galle—was rather favourably received, on account, it is insinuated, of one face of the *bifrons Deus* representing the mathematical, and the other the physical discoverer: but this was opposed as being too indiscriminate. A friend of elegant mind thought Minerva would be appropriate, despite of a little Pallas being already in the way: and Hyperion, the offspring of Uranus and Terra, presented himself, not as the Sun or the Sun's father, but in capacity of Ὑπερίων, the *Transcender*, or, more literally, *above us going*. An Oriental scholar suggests a higher flight into antiquity, and there picking up Sanchoniatho's Elioun, the Hypsistus of Philo-Byblius, because he was the reputed parent of Uranus: but, in quoting these gentlemen, we trust we are not poaching on Ephraim Jenkinson's ground, or otherwise disturbing the shade of Goldsmith. Le Verrier himself sanctioned Neptune, the designation conferred by the *Bureau des Longitudes* at Paris; and the sea-deity instantly gained the largest number of votes, especially as the type was a trident made from a monogram of the initials of the French geometer. So the symbol of Uranus identifies Herschel; and by such arrangement the discoverer of a planet will ever be held in honour and remembrance, whatever may be the appellation of the discovery.

But, though most of the *e merito* astronomers signified their adhesion to Neptune, he was not allowed to walk the course. A terse Northern Professor, overlooking the marine deity's alliance with us in ruling the waves, thus perorates:—"The god is degraded, in the eyes of a Briton at least, by the disturbing influence of low and vulgar associations. For who can hear of Neptune as the name of the new planet, without being reminded either of the wooden sea-god that he has seen, trident in hand, in the poop of many a vessel, or of his living representative in the person of a sailor at the ceremony of Crossing the Line, or in some pantomime at Sadler's Wells; or, it may be, of some Newfoundland dog who rejoices in the name of Neptune?" And is this the amount of what a British Professor knows of Neptune! The unkindest cut of all, however, was given by our gifted friend M. Arago, who publicly pledged himself (*je prends l'engagement*), whatever might happen, not to call the stranger by any name except that of Le Verrier: a decision at which Le Verrier, who

was present at the sitting of the Academy, says he was somewhat startled (*j'ai été un peu confus*). The United Service to a man will, we expect, stick to Neptune and the Trident.

Thus endeth our story of the new Planet. To be sure Mrs. Borron, of Croydon, has publicly stepped forth and insisted that Neptune is not the body sought for by Le Verrier's investigation, but a planet which happened accidentally to be in the field of the telescope when Dr. Galle made his scrutiny. Since this assertion was publicly made, our brethren on the other side of the Atlantic—aye, and even some of Le Verrier's own learned compatriots—arrive at the same conclusion, and have supported Mrs. Borron's paradox by $x + y - z$. There are certainly perturbations still to account for; and the mean distance of Neptune proving to be much less than the limits assumed, may indicate a change in their very character—but surely the assault is premature. The erudite Professor Peirce communicated to the American Academy of Sciences, 16th March, 1846, the computations of Mr. Sears C. Walker, who had detected a missing star in the *Histoire Céleste Française*, observed by Lalande, on the 10th of May, 1795, near the path of the planet Neptune, at that date, which may have been possibly the planet in question. Mr. George P. Bond, of the Cambridge U.S. Observatory, joined in the scrutiny of all the data; and the conclusion which these scientific gentlemen have seriously arrived at is, that *the planet Neptune is not the planet to which geometrical analysis had directed the telescope.* OHE!

Let the whole of the *ἱερα φαλαγξ* of cosmopolite Geometers look well to this delusion, and unveil the happy accident to which the discovery of Galle is owing; let them kindly remember what an amount of probable error was attached to the conclusions, and find out whether the new planet is capable of causing the perturbations in the motions of Uranus complained of; let them tell how queerly Lalande allowed Neptune to slip through his fingers, after catching him on the 8th and 10th of May, more than 50 years ago; and finally let them fully revise the now-faulty elements of the complicated motions before them. There will be time for all this before A.D. 1850; so that the approaching decennium may pass in unity, peace, and concord.

NO. II.

REPORT ON BARLOW'S FLUID OBJECT-GLASS.

(*Referred to on page* 158.)

[From the Proceedings of the Royal Society for 1833-4.]

December 12, 1833.

FRANCIS BAILY, Esq., Vice-President, in the Chair.

The Reports received by the Secretaries, from Sir John Herschel, Professor Airy, and Captain Smyth, on the Fluid-lens Telescope constructed for the Royal Society on Mr. Barlow's

principles, were, by direction of His Royal Highness the President and Council, read to the Society at this Meeting.

Sir John Herschel's Report.

I have seen Mr. Barlow's telescope at Cambridge, and examined it on several objects, in a very fine night, the 25th (if I remember) of June. As I have now no time to give it any further trial at Slough, (where I have no longer, either, any achromatic telescope of sufficient power to compare it with, all my apparatus being dismounted and in course of packing,) I will here state in few words, as *my* report on it, all I could then collect relative to its action.

1. *Achromaticity.*—Mr. Barlow's telescope is remarkably free from the dispersion of colour; very much more so than I could have expected from the nature of the correcting medium, and nearly or quite as much as could be desired.

2. *Light.*—The great aperture is very efficient under moderate powers on faint objects, and it concentrates the smaller stars well, and would, I have no doubt, show the larger nebulæ, &c., and be well available as a *sweeping telescope.*

3. *Distinctness.*—Very good with powers under 100 or 150; but, on the occasion on which I tried it, it seemed to break down under high powers, and there was evidently a considerable want of correction of spherical aberration. As I had no opportunity of trying it in different temperatures, I cannot say whether this want of correction might not disappear in a different temperature—it was about 65° when I looked through it,—neither could I ascertain whether this arose from the glasses not being at the right distances, there being no means, or the means not having been explained to me, by which the correcting lens could be got at, to shift it.

4. A very troublesome degree of colour out of the centre of the field.

This report is of course too meagre and imperfect to conclude much from, but, as both Capt. Smyth and Prof. Airy have examined it in much detail, I the less regret that my present circumstances will not allow of my going further into the subject.

J. F. W. Herschel.

July 23, 1833.

Professor Airy's Report.

From the pressure of business, I have had fewer opportunities of trying the telescope than I could have desired. The absence of bright planets also has prevented me from attending so much as I wished to what I regard as the most important point in this construction, namely, the correction of colour. I have, however, had one excellent opportunity of observing the moon, and have observed several stars, single and double, and do not think that my opinion could have been altered by a greater number of observations. The correction of colour is not complete, but it is much more nearly complete than I expected, and very much more so than in a smaller telescope of Mr. Barlow's construction which I tried several years since. The colour is so far removed that it is not offensive till a power of 300 is used. But with regard to this colour, there is one point of great importance to be noticed by any person who shall try the telescope in future. It is that, in consequence of the separation of the object-lenses, the only part of the field which can possibly be free from colour with a common eye-piece is in the line passing through the centres of the two object-lenses; and

that from the present imperfect centering, this line falls actually out of the field of the highest power (or quite on the edge). An eye-piece of a different construction and adjusted with greater care is necessary before any positive decision can be given. With regard to the definition of a star, it is not at present good, and the telescope is decidedly incompetent to separate any close star; but I regard this as a fault in the making of the surfaces, to which any telescope is liable, and which does not interfere at all in my estimation of the value of the new principle of construction. I know not how far a circumstance mentioned by Mr. Dollond (the alteration of spherical aberration with an alteration of temperature) may account for this; but so much of the irregularities are cut off by cutting off the external ring of the object-glass, that I have no doubt of its being due principally to the figure.

My opinion is, therefore, that a larger telescope, as good of its kind as the present, would be very useful for nebulæ, &c.; and that, if freed from defects which do not appear to belong to the construction, it might be equal to any astronomical work except the examination of bright planets.

I have had the advantage of trying the telescope once in company with Sir John Herschel, Sir David Brewster, Mr. Cooper, Dr. Robinson, and Professor Hamilton, and their opinion, upon the whole, coincided nearly with mine.

I beg to suggest the propriety of attaching a finder to the telescope, as much time is lost in seeking for any object.

G. B. Airy.

June, 20, 1833.

Captain Smyth's Report.

I beg you will inform His Royal Highness the President, and the Council of the Royal Society, that I have this day packed up the fluid refracting telescope of which they have done me the honour of asking my opinion, and that it will be forwarded to Professor Airy without delay. It might indeed have been sent to Cambridge sooner, but that I waited for the first quarter of the present moon, to test the light and the performance of the instrument; but I regret that, though I was constantly upon the spot, the weather has prevented my having an opportunity of catching her till she was past her dichotomy, and consequently too glaring for the purpose.

On the arrival of the telescope, it was carefully unpacked, and immediately mounted, for the moment, on the lower slab of the revolving roof of my polar-axis room. It was fitted by its two pivots to the iron crutch which was sent with it, the upper parts of which were cut into Y's: the inner end was supported by Mr. Dollond's ingenious "eye-end stand." The instrument, however, was liable to tremor, both from the motion of the roof and the floor; but it enabled me to examine a few objects while poles were being prepared to form a better stage outside the Observatory. And I should remark, that it was arranged with Professor Airy, who favoured me with a visit on the occasion, that my experiments were to be entirely confined to the performance of the telescope, while he would investigate its principle. My portion was to be governed by direct comparisons with my refractor, as a standard from which to assume the relative merits of the two. That instrument has a double object-glass of $5\frac{9}{10}$ths inches clear aperture, and $8\frac{1}{2}$ feet focal length; a space which I have good reason to think is accurately proportioned to the densities of the crown and flint

glasses: and, notwithstanding the magnitude of the diameters, the curves of the lenses seem in tolerably exact chromatic and spherical aberration throughout. It may therefore be presumed to be a more severe reference than the dimensions alone would suggest.

The temporary stage alluded to, outside the Observatory, consists of two upright beams of fir, firmly driven into the bed of gravel which forms the substratum of the garden, and a cross-bar, strongly screwed, supports the iron crutch with its Y's. This is erected close to a platform and pier, which were built for some magnetic experiments, and afforded great facility in attending to the outer lens, and augmenting or diminishing its aperture. While looking towards the south, it commanded from nearly a horizontal view to above 60° of elevation; and by unshipping it, and turning it northwards, it swept the polar region. Such being the means, it remains faithfully to report what I observed, regretting, at the same time, that the weather has continued mostly unfavourable.

Monday, Feb. 25, 1833.—The evening cleared off, and was very fine from 8 till nearly 11 o'clock P.M. At 7, I placed the instrument on its stand; at 9, the thermometer was 37°·6, the barometer 29·32, and the hygrometer ·771; the wind was at S.E.

1. *The Moon.*—The examination of the lunar cavities and shadows was rather unsatisfactory. Under the powers 250 and 400, it bore the whole aperture; but with 90 and 150, there were two spectra, one of which haunted the centre. In definition, the fluid was excelled by the flint-glass, both instruments being very steady.

2. *The great Nebula in Orion.*—This mass was seen very fairly with the whole aperture; and the trapezium was beautifully distinct under all the powers except that of 400. From the examination of this object, the best performance seems to be with the eyepieces 150 and 250. The relative light of the flint-glass and the fluid-refractor's, when the latter was reduced to six inches of aperture, appeared very nearly equalized.

3. *Venus.*—This trial was altogether unsatisfactory, from the strong irradiation and the quantity of loose light. The planet was, however, low down in the west, in a stratum of mist. The only power used was the one of 90 times; but there was a great defect in distinctness.

4. *Rigel.*—This star was in the S.W., and rather low; it was, therefore, as might have been expected, surrounded with teasing rays, through which I had some difficulty in detecting the small companion. The star had a spurious but broken disc, and was full of colours in every part of the field except the centre, where they were partially destroyed. Powers 150 and 250.

Tuesday, Feb. 26, 1833.—At 9 in the morning, with the thermometer at 38°·8, and the hygrometer ·798, I examined an enamelled watch-face, which is firmly fixed upon a distant chimney of solid construction; and, though the solar focus could not be used, I considered sufficiently distinct vision would be obtained to test the achromaticity of the telescope. The plate itself bore the trial better than did the edges of the chimney-sides, where the focus could not be adjusted so as to prevent the alternate production of light green and purple mist, as the eye-tube was pushed in or out: and these colours scarcely disappeared, even when brought into the centre of the field of view. Some of this might probably be corrected by adjusting the fluid-lens for near objects: and Professor Barlow writes to me, that this can readily be done; but that he took off the screw head, by which it

is effected, to prevent mere lookers-on from deranging the instrument. The watch-face being upon a dark ground, I played the eye-tube till I procured a spurious disc over it, by which I was satisfied that the centering was very nearly accurate.

I then left the telescope *in statu quo*, and at half-past 12 again inspected it, the thermometer being 47°·6, the barometer 29·23, the hygrometer ·789, and the wind S.S.W.; particulars which I carefully noted, for a reason which will presently appear. The watch-plate was now considerably plainer, and its figures more sharp and distinct; but the focus required shortening in; and, though there was less colour than before, I was surprised to find it verging to the prismatic extreme, and tinged with red; a circumstance which ocular foci, or the distance, would hardly account for. I repeated the examination in the evening, when the thermometer was 45°·4, and the hygrometer ·790. I now found that the focus required lengthening; but the vision was at its best, and the colours had almost vanished, though a foggy spectrum was perceptible at times. High powers of course did not agree with so near an object; but they were used without greatly distorting the image.

Thursday, Feb. 28, 1833.—The weather was very fine from 10 to 12 P.M., though the wind blew occasionally in hard squalls from the S.W. The instrument appeared but little affected, and yet the observations were rendered unsatisfactory by the frequency of these gusts. At 11, the thermometer was 38°·4, the barometer 29·45, and the hygrometer ·723.

1. *σ Orionis.*—Saw 8 of the 10 stars which compose this cluster, but not sharp. This situation was unfavourable, it being two hours and a half off the meridian, and the S.W. quarter of the heavens was hazy. The power used was 250.

2. *ζ Orionis.*—This, of course, was very plainly seen; but I fished it up for its definition. The large star had a formidable nimbus, yet it did not prevent the increase of dark vacancy on raising the magnifying powers. There was much less loose light than I expected, and the small star was palpably of a pale blue tint.

3. *Rigel.*—This star was now too far in the S.W. to be made much of: it was tremulous, and greatly irradiated under power 250. The companion was not visible, and there were two troublesome spectra.

4. *Saturn.*—The body of the planet bore magnifying powers, and showed the thin silver line of ring which now appears, without distortion, but certainly without sharp definition. I could only perceive two of the satellites, while with the flint-glass refractor I saw three. The whole aperture was too much for the instrument, and it was therefore cut off to six inches.

5. As the north was now the clearest part of the heavens, at about 11 o'clock the telescope was turned to that direction. The pole-star and its companion were seen very distinctly; even under the lowest power. This, of course, I expected; but I found that it was also viewed, on both sides of the object-glass, with much less colour than the other tests I had been looking at.

Monday, March 4, 1833.—This was the best night I had yet had, and it continued very fair till nearly midnight. I was somewhat troubled with dew, but the instrument was free from tremors, and worked as well as its temporary mounting could admit of. The temperature stood thus:—

	8 *o'clock.*	10 *o'clock.*	*Midnight.*
Thermometer	45°·5	43°·6	40°·5
Barometer	29·85	29·86	29·89
Hygrometer	·740	·737	·728

1. *Rigel.*—This star was greatly discoloured at the edges of the field, and was accompanied by a singular spectrum, which was not destroyed by being brought into the centre. I caught the companion by glimpses, but it was immersed among strong rays. The powers used were 90 and 250.

2. *Sirius.*—This brilliant star was still more discoloured than Rigel on either edge of the field of view, and had a continuous production of rays, which in the centre surrounded the star, but at the sides preceded and followed it, like the wings seen where a flint-glass is not homogeneous, but fainter. These irradiations, as well as the dispersed light, were considerably cut off by diminishing the aperture of the outer lens. The powers used were 90 and 150; and I tried with 400 to raise a disc, but it was altogether too much for the object.

3. *σ Orionis.*—Saw the whole of the ten stars of this group, but with great difficulty, and, if the term may be used, under a dim definition. Indeed, had I not practically known the object, I am doubtful whether I could have made out the middle stars. It should, however, be also stated, that it was nearly three hours to the west of the meridian.

4. *The great Nebula in Orion.*—I placed the whole aperture upon this object; and, though the moon was nearly at full, I easily made out its outline, as well as that of its companion. But the trapezium of stars, under high powers, was more distinct with an aperture of 6 inches than when the whole was applied. I could make out only four stars in this spot; it will be recollected, however, it was now three hours past the meridian, for the time of its transit will not allow of earlier experiment This I regret, because so fine a constellation, from its composition and place, offers in itself a *thesaurus* of astronomical tests.

5. *γ Leonis.*—This beautiful double star was remarkably well seen, being nearly on the meridian. There was, however, much false light, but it did not hinder the colours being seen: the large star was slightly red, and the small one a Saxon green. The powers used were 90, 150, and 250.

6. *ω² Leonis.*—This was a test which, in the deficient arrangement of the apparatus, I could not manage; but, notwithstanding there was mnch dispersed light, I should pronounce that with power 400 I saw the star elongated, and different from the other two *omegas.*

7. *The Præsepe.*—An examination of this cluster was very favourable to the defining powers of the telescope, and its general distribution of light. I tried it under the eyepieces 90, 150, and 250.

8. *ζ Cancri.*—With some difficulty I made out this object to be triple, under a power of 250: that of 400 broke the rings of the spurious discs with disagreeable rays, so as to confuse the whole vision.

9. *Saturn.*—The planet was about two hours and a half to the east of the meridian when I placed the telescope upon it. It was tolerably defined, but with *muddy* edges, though it bore magnifying pretty fairly. I saw two satellites steadily, and a third by glimpses; and this was all I

could do with my own telescope at the time of transit. The ring resembled a thin silvery bar lying equatoreally across the planetary disc, and was sharper than the body of Saturn.

Wednesday, March 20, 1833.—I had now intended to wait for the first quarter of the new moon; but the night proved so fine and dark, that I re-examined some of the former tests, and observed some new ones. There was a light N.W. wind, and the temperature was thus:

	9 *o'clock.*	*Midnight.*
Thermometer	37°·7	34°·4
Barometer	30·01	30·00
Hygrometer	·680	·670

1. *The great Nebula in Orion.*—This was now three hours and a quarter over the meridian, and yet was seen in great beauty and distinctness under the whole aperture, with eyepieces 90 and 150. The trapezium was examined very closely with 250 and 400, which last it bore better than it had yet done; but only four stars were visible.

2. σ *Orionis.*—All the stars of this group were perceptible under the power 250, but they had the appearance of being seen in a second-rate reflector; so that I know not how a micrometer would work upon this instrument.

3. *Venus.*—The crescent which this planet now forms was better seen than heretofore, but an unseemly quantity of light still attended it; and under the higher powers the colours were intolerable. When, however, the focus of power 90 was nicely adjusted, and the planet brought exactly into the centre of the field, it was a beautiful object, despite of a secondary spectrum. The aperture was reduced, and I did not find, either now or on other occasions, that this sensibly affected the ocular focus.

4. γ *Leonis.*—This brilliant object was distinctly seen, and the dark vacancy between the stars increased more than did the spurious discs, while the magnifying powers were being raised, though much loose light and irradiations were thereby produced. And it is singular that the separation was improved by my placing a central disc of card-paper, two inches in diameter, on the outer lens.

5. *Messier's 46th Nebula.*—This was very fairly resolved into stars, and better with the whole than the reduced aperture. Eyepiece 90 showed it easily, but the higher powers gave it a very *turbid* appearance. The preceding cluster was brilliant.

6. α *Leonis.*—This star had a bunch of disagreeable rays shooting from it; and the light, when under the best adjustment I could give the focus, was curiously thrown to the northward. I was able, however, to raise a tolerable disc, and the small star at a little distance from Regulus was unusually distinct.

7. 24 *Comæ Beren.*—I pointed to this remarkably pretty object to test the colours, and very readily perceived the large star to be of a bright orange colour, and the small a sea-green. This was one of the best sights I had yet had, and on the whole was satisfactory.

8. ι *Leonis.*—This, though a very close and unequal double-star, was well shown, yet at times the stray light would obscure the companion. The large star was fairly figured, and the small seemed about the 10th magnitude, and of a greenish hue. It formed a fine test.

9. *Saturn.*—I had a good trial of this planet; and though the powers 90, 150, and 250, were borne, the disc was certainly not well defined. The ring is still a mere *bar* lying across the equator: it was very well shown, as were also three satellites. When I applied the power 400, the whole field was strewn with harsh light.

10. γ *Virginis.*—This interesting star, though now so exceedingly close, was made double with 250, and very well shown; but with 400 there was great tremor and irradiation, so that the discs were often confused into one.

Saturday, March 30, 1833.—After a continuance of bad weather for several days, it cleared off a little; but in the mean time I had missed the favourable phase of the moon, for which I had been waiting. I therefore closed my examinations with the following one:

The Sun.—From the extreme volatility of the sulphuret of carbon, I was fearful of its expansion, and therefore had not yet turned the telescope upon the sun, lest the condensation of the solar rays, at the place where they traverse the fluid, should prove too much for the lenses. But on mentioning this apprehension to Professor Barlow, that gentleman assured me that an exposure of from five to ten minutes could do no mischief. I therefore this day reduced the aperture to three inches, and directed the instrument to the solar disc, when, sweeping over the luminary for about three minutes, I found the surface was quite clear of spots. On turning from it, I drew out the eye-tube, and looking at the fluid, perceived that the bubble was considerably diminished, but not so much as I had expected. This was the only time that I exposed the telescope to great heat.

These are the only experiments I have been able to make; and the season of the year, together with the inefficiency of the apparatus, have certainly prevented me from assigning exact limits to the performance of this telescope. Still, as I had immediate reference to one of the best refractors extant, I may add the following conclusions, premising, that I have not constantly noted down the performance of the latter upon each test, because my end was to pronounce upon the fluid object-glass. I should also observe, that the magnifying powers of both the instruments were equally matched, and their apertures were generally proportioned to nearly six inches: the eye-pieces were thus:

Fluid refractor	90	150	250	400
Flint-glass ditto	93	157	240	416

From the result of my observations, it has struck me that this ingenious principle has strong claims to consideration for its valuable optical powers, but that, in the present stage, it is more adapted for stars than for planets; and should the application of it be tried on a larger scale, it might be made with sufficient illumination to examine the high-class nebulæ; a branch of practical astronomy which is now nearly shut against refractors. The defining power does not strike me as being so good as the light, nor does the achromatism seem to be perfect. Yet I should mention the want of focal and mechanical arrangement; that the only adjustment I had for distinct vision was by the hand, with the sliding eyepiece tube; and that slight derangements might be occasioned by the mounting and dismounting of the great tube, however carefully it was attended to.

I cannot but suspect that the performance of this telescope is affected by temperature, and that severe tests in the summer months might afford different conclusions to those which I have arrived at; but as I considered my opinion was desired on the instrument in its present state, I took no

means for applying artificial heat. And, perhaps, the secondary spectrum which haunts the field might be mitigated, and the prismatic colours destroyed, by an alteration of the distance between the fluid and outer lenses;—the same consideration prevented my applying for a screw, by which it might have been effected.

But there is one condition of the instrument which, if correct, would be of greater importance than the rest, as connected with this Report. It strikes me forcibly, from the several effects I observed, that the focus has been cut too short; a defect which would seriously affect the spherical aberration of the outer or object lens and its dispersion: and this would account for the fluid refractor not performing better than the flint-glass one, without impugning the corrective powers of the sulphuret of carbon, or its skilful application by the scientific Professor.

W. H. Smyth.

April 4, 1833.

No. III.

SIR JOHN HERSCHEL'S ORBITAL REVISION OF γ VIRGINIS.

(Referred to on page 357.)

R A $12^h\ 33^m$ N P D $90°\ 31'$. The apparent perihelion passage of this interesting double-star took place in the first half of 1836, and the appulse of the two stars proved (as predicted) so very close as to cause it to appear as a single star to all but the most powerful and perfect existing telescopes under the most favourable circumstances. In no part of the interval from 1835.971 to 1837.545, both dates inclusive, was it possible to observe any *certain* elongation of the united discs with the 7-feet equatoreal, capable of being in the smallest degree relied on for a measure. It should be observed, however, that, owing to the influence of the arid and heated sandy tract intervening between Feldhausen and Table Bay, to the northward of the former station, it was rare to procure even moderately tranquil images of stars situated so near the equator. In consequence almost all the *measures* obtained of this star were procured only by the most obstinate patience and perseverance in waiting for favourable glimpses, occurring among long intervals of confused and agitated definition, as the notes appended to the individual night's results sufficiently show.

During the interval between the dates above mentioned, the star was frequently examined. On the night of the 21st December, 1835, it was kept in view nearly an hour, trying all sorts of methods to divide it, but in vain.

"I see the disc round and sharp, though tremulous, and the rings, &c. all in motion;—no power I can apply gives me the least certainty of an elongation. To this telescope, with its highest power and in good action, the star is single. (7 feet Equ.)"

Again, on the 23rd December, 1835, it was examined with all the usual powers up to 400.

"Definition very far superior to any I have ever had of this star since I have been at the Cape. It's disc, at the best moments, is seen steadily round. I can no way succeed in dividing it. When the focus is over-urged, to give the smallest disc, it was seen a little wedge-formed downwards, but turning the telescope

in that state on Spica the same appearance was seen. This observation was made just before and during the morning twilight, and as the star gained altitude the definition continued improving."

"January 10, 1836. In the best moments I fancy I see a very trifling elongation—roughly about the direction of the meridian with 279 and 300." [400] "Worked at it a full hour, turned the object-glass and its cell in the tube half a turn round with remarkably good effect."

"February 9, 1836. In the commencement of twilight, in the thin scud of a black south-easter, which is beginning to bring up its cloud after a cloudless night, I feel pretty sure that there *is* an elongation as indicated. Pos 18° 50′ a single measure, with the lowest weight a *measure* can have."

February 18, 1836,—20-feet reflector. "Viewed γ Virginis with powers 320 and 480, which gave round discs and very good definition; but could get no indication of its being double. Far better defined than γ Centauri," which had just before been viewed with the same instrument, and which was seen well elongated, and in momentary glimpses separated with 320 and a triangular aperture.

May 3, 1836. "With the utmost patience and the greatest difficulty, from the violent agitation of the star, I think I have a measureable elongation. Pos. = 5° 30′" (by a single measure).

May 12, 1836. "γ bears No. 4 better than usually No. 3, and I am almost sure it is elongated a very little." Pos. = 3° 30′ (by a single measure marked $\pm$).

"January 14, 1837. Being a most glorious night, I waited till 10h. 20m. Sid. T., and then attacked γ Virginis. Capitally defined, though *catching*. Bore No. 4 well In some moments I keep suspecting an elongation from *f* to *p*, but at others it is quite round. Put on aperture $2\frac{1}{2}$ inches—a beautiful planetary disc without rings. If it be elongated the major axis is not more than $\frac{1}{10}$ greater than the minor. In short, I came to the conclusion that, for anything I could see to the contrary, the star is single. In this state, turned the telescope on γ Centauri with the powers No. 3 and No. 4 distinctly elongate, consequently I conclude that the distance of γ is at most $\frac{1}{2}$, perhaps $\frac{1}{3}$, that of γ Centauri, which is certainly under 1″."

"April 24, 1837. The best sight of γ Virg. I have ever got since my residence at the Cape. Quite round with No. 4, nor the smallest sign of elongation that I can perceive. The rings *brandish* a little, but slowly, and the disc *moulds* a little, but it is altogether finely defined, and examined quite at ease. 2^{h} west of merid."

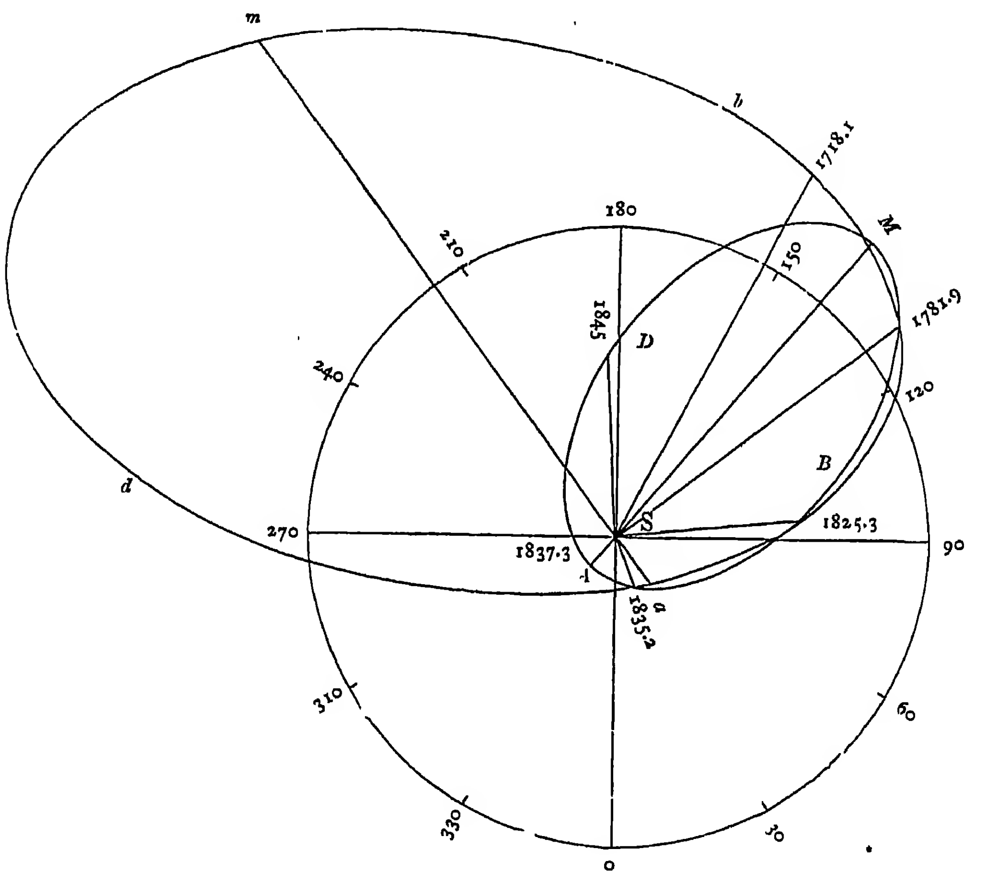
"May 18, 1837. Scarcely a doubt of elongation. Long contemplated with No. 4. It is surely not round, and position, by 3 measures, = 202° 0′. (Brandishing but a *disc*.) After an hour's rest of the instrument much less evidently elongated. Still I think it *is* so, but am by no means so certain as when first seen."

"June 27, 1837. I am almost certain of an elongation 60° $\pm$ n f or s p; but the star will not bear No. 4. No 3 shows it round, but wants power. A reading (such as it was) of the micrometer taken, gave Pos. = 211° 40′.

"July 19, 1837. Certainly elongated. Of this I have no doubt. The star is better defined than I ever saw it at the Cape under No. 4. Definition excellent—for *this* star unequalled. Pos. by a single reading 210° 20′.—Further examined. When best in focus, *i. e.* when disc reduced to its least magnitude, *perfectly round*. Turned the telescope on β Corvi and δ Corvi. In similar state of the focus I see the same elongation in the same direction, but when perfectly adjusted the discs are quite round, and so is that of γ Virginis."

Such is the history of my observations of this star during its perihelion appulse. The *time* of the perihelion passage appears to have been retarded nearly two years beyond that predicted in my first calculation of the orbit, the elements of which, however, as subsequent observation has

shown (with the exception of the excentricity), are incapable of representing the motion of the stars far beyond the limits for which they were calculated, however completely the conditions of it were satisfied by them within those limits. The reason of this abandonment of the old supposed orbit, and the substitution for it of one differing so very widely from it in all but the single element above mentioned, which the further progress of observation has necessitated, will be readily understood by the inspection of the annexed figure, in which M D A B represents the actual orbit



described by the one star about the other at S, and *m d a b* the orbit represented by my first elements. It will be seen that the two orbits are almost exactly coincident over all that portion of each which corresponds to the interval embraced by the observations from 1781 to 1835, within which alone micrometrical measures had been at that time obtained—the actual or smaller ellipse being curiously packed within the other in the manner of an osculating curve, intersecting it in four points, and deviating from it in the intervals between them by a quantity much too small to excite any suspicion of error at that time.

This is not the first by many instances in the history of scientific progress where, of two possible courses, each at the moment equally plausible, the wrong has been chosen. But in this case, the adoption of the larger ellipse seemed to be necessitated by the positions concluded from the observations of Mayer in 1756, and of Bradley in 1718, which it appeared desirable, if possible, to conciliate.* Now, however, that micrometric measures have been sufficiently multiplied, positions obtained like that of Bradley by mere allineations with distant stars, one eye being at the telescope, the other directed to the heavens; or, like that of Mayer, by a still more insecure method, viz., by independent differences of R A and P D taken from a catalogue, can by no means be admitted among the normal data to be used in the determination of elements.

The lapse of a very few years sufficed to show that the movements of the stars are performed with much greater rapidity, and in a much smaller orbit than that at first assigned to them. Elements of their orbit have been computed from time to time by Messrs. Maedler, Encke, Captain Smyth, the late lamented Professor Henderson, and, recently, by Mr. Hind; also by myself in 1843 (to which reference has been made by Captain Smyth, in his useful and elaborate work, "A Cycle of Celestial Objects," ii. p. 281). It is not a little remarkable that all these calculations agree in assigning almost precisely the same value to the *excentricity* (physically speaking, the most important of all the elements) as that resulting from my earlier calculations, though they differ materially from each other in the position of the orbit in space, and especially in the period; Captain Smyth's period still extending to 196 years, while, with a certain combination of other elements, so low a period as 124 years seemed to myself not absolutely excluded; at least if *all* the micrometrical observations on record be considered as entitled to equal credit.

This, however, for reasons which will presently appear, I consider more than questionable; and as the complete establishment of the elliptic motion of this binary star is a point of high importance—one of the great facts indeed of modern astronomy—a re-investigation of its orbit, founded on careful examination of all the recorded measures, will not be deemed irrelevant in this place. To this end there are assembled in the following table the mean results of all the recorded observations of its angles of position (micrometrically measured) which had come to my knowledge up to September, 1845.

* In my earlier investigations of the orbit of this star, a mistake of 10° was committed in reading off from a celestial chart the angle of position of the two stars α and δ Virginis, with which Bradley compared the double star. Mr. Henderson has rectified this mistake. In consequence of it the agreement of my first orbit with Bradley's observations is only apparent, my calculated position for 1718 being 159° 17′, and the position actually resulting from Bradley's comparison (with the due corrections for precession and *ocular equation*) being 150° 52′, instead of 160° 52′, as set down in my paper (Mem. Ast. Soc. 1832). It is remarkable, however, that Mr. Henderson deduces from his own elements a position of 161° 16′ for the epoch in question, differing by less than 2° from my computation, though starting from such widely different elements. Both are erroneous, however, and this important observation of Bradley's will be found very satisfactorily represented by the elements about to be given further on.

No.	t	θ	Obs.	No.	t	θ	Obs.	No.	t	θ	Obs.
1	1781.89	130° 44′	H	20	1832.30	69° 55′	D	39	1837.21	265° 27′	Sm
2	1803.20	120 15	H	21	.40	71 26	Sm	40	.41	257 55	Σ
3	1820.29	105 15	Σ	22	.52	73 30	Σ	41	.48	256 24	M
4	1822.25	103 24	S.h	23	1833.18	61 46	h	42	1838.08	237 28	h
5	.35	102 44	Σ	24	.30	61 9	D	43	.28	235 41	Sm
6	1825.32	96 58	S	25	.33	68 10	Sm	44	.32	233 26	D
7	.32	97 55	Σ	26	.37	65 32	Σ	45	.43	231 5	O
8	1828.35	90 30	h	27	1834.30	47 9	Sm	46	.43	229 12	M
9	.38	91 30	Σ	28	.31	46 48	D	47	1839.32	214 35	D
10	1829.22	87 48	h	29	.38	51 40	Σ	48	.40	217 12	Sm
11	.39	88 17	Σ	30	.41	39 0	h	49	1840.38	205 43	D
12	1830.38	82 5	h	31	.84	33 26	Σ	50	.45	205 42	O
13	.39	81 29	D	32	1835.11	21 27	h	51	1841.34	200 8	D
14	.59	82 10	B	33	.38	15 29	Σ	52	.35	200 11	M
15	1831.33	77 12	h	34	.40	14 59	Sm	53	1842.34	197 25	A
16	.35	78 12	D	35	1836.28	350 9	D	54	.41	194 59	D
17	.36	80 55	Σ	36	.34	340 48	Sm	55	1843.38	191 36	Sm
18	.38	77 54	Sm	37	.41	331 34	s	56	1845.34	185 24	Sm
19	1832.27	70 20	h	38	1837.20	280 25	E				

In this table, the positions set down on the authority of each observer (with exception of those of M. Struve in 1834) are the means of his recorded observations for each year. The first column contains a number for reference; the second, headed t, the mean epoch of observation; the third, headed θ, the angle of position corresponding, and the fourth, the initial of the observer's name.*

If these positions be projected on a chart of engraved squares for the purpose of mutual intercomparison, in order to the construction of an interpolating curve representing the most probable course of the variation of this element during the whole interval of observation, it will at once be apparent that the observations of M. Struve from 1828.38 to 1834.38, both inclusive, cannot possibly be used in conjunction with the rest of the series. A curve drawn through the points representing these observations, separates itself gradually and systematically from that which expresses with the utmost consistency and regularity the general course of the movement as

* A. Airy; B. Bessel; D. Dawes; E. Encke; H. Sir William Herschel; h. Herschel, junior; M. Maedler; O. Otto Struve; S. South; Sm. Smyth; Σ. Struve; s. Sabler.

deduced from all the other authorities, the amount of deviation at length reaching no less than 9°, after which it ceases abruptly, the subsequent results of M. Struve's measurements, as well as those previous to 1828, being in good accordance with the rest, on their whole evidence, as so represented. This is clearly a case in which no middle course can be taken. To include these observations with the rest by a system of taking means, would be to sacrifice the validity of both series, and to mar the chain of data at a most important point of its history. I shall, therefore, proceed independently of them, and as it would be manifestly unfair in an investigation of this nature arbitrarily to retain some and reject others, among a series of results recorded by the same observer, shall leave, in what follows, M. Struve's measures out of consideration, proposing on another occasion to make them a subject of especial inquiry with a view to the determination of an orbit resting solely on their evidence.* The observation No. 53 having been made with a double image micrometer, giving confessedly distorted images, and standing, moreover, in irreconcilable contradiction to the general evidence of all the other observations from 1840 to 1845 inclusive, is also rejected.

The following tables express, the first, a series of mean epochs and positions deduced by taking means of the observations of each year (with the exceptions specified); and the second, the course of the interpolating curve which, on a general and perfectly impartial consideration of all their evidence, appears to me to exhibit the most probable course of the movement throughout (as deduced from observation without the aid of any elliptic hypothesis), and more especially during the year 1836, when, owing to the excessive closeness of the stars, all micrometrical measurement was, if not precluded, at least rendered liable to errors of unusual magnitude.

TABLE OF MEAN OBSERVED EPOCHS AND ANGLES OF POSITION OF γ VIRGINIS.

t	θ	Observ. used.	t	θ	Observ. used	t	θ	Observ. used.
	° ′			° ′			° ′	
1781.89	130 44	1	1832.32	70 34	19, 20, 21	1838.31	233 22	42, 43, 44
1803·20	120 15	2	1833.27	62 2	23, 24, 25			45, 46
1822.25	103 24	4	1834.34	44 19	27, 28, 30	1839.36	215 53	47, 48
1825.32	96 53	6	1835.25	18 13	32, 34	1840.41	205 42	49, 50
1828.35	90 30	8	1836.31	349 58	35, 36	1841.34	200 7	51, 52
1829.22	87 43	10	1836.41	331 34	37	1842.41	194 59	54
1830.44	81 55	12, 13, 14	1837.30	267 25	38, 39, 41	1843.33	191 36	55
1831.35	77 46	15, 16, 18				1845.34	185 24	56

* This only applies to the observations marked Σ. Those by M. Otto Struve, with the Pulkova telescope, are of course retained; as also the highly important joint observation No. 37, which cannot be dispensed with.

TABLE OF INTERPOLATED EPOCHS AND ANGULAR VELOCITIES OF γ VIRGINIS FOR ANGLES OF POSITION FROM 10° TO 10°.

θ	t	$-\frac{d\theta}{dt}$	θ	t	$-\frac{d\theta}{dt}$	θ	t	$-\frac{d\theta}{dt}$	θ	t	$-\frac{d\theta}{dt}$
°		°	°		°	°		°	°		°
130	1783.41	0.48	50	1834.04	18.2	330	1836.37	62.0	250	1837.72	34.5
120	1808.61	0.60	40	1834.51	23.3	320	1836.53	67.2	240	1838.04	28.0
110	1816.60	0.99	30	1834.90	29.4	310	1836.67	70.8	230	1838.46	20.9
100	1824.20	1.74	20	1835.28	33.3	300	1836.81	72.0	220	1839.06	14.9
90	1828.47	3.1	10	1835.52	37·0	290	1836.95	71.2	210	1839.94	9.6
80	1830.90	5.1	0	1835.78	41.7	280	1837.09	66.4	200	1841.34	5.6
70	1832.48	8.2	350	1836.01	47.6	270	1837.25	55.6	190	1843.84	3.2
60	1833.40	12.7	340	1836.20	55.6	260	1837.46	43.9	185	1845.50	2.7

Setting out with this latter table as a basis of calculation, I find the following elements:—

Excentricity	$e = 0{\cdot}87952$
Inclination to the plane of projection	$\gamma = 23°\ 35'\ 40''$
Position of ascending node	$\Omega = 5°\ 33'$
Angular distance of Perihelion from Node on the plane of the orbit, or true angle between the lines of Nodes and Apsides	$\lambda = 313°\ 45'$
Epoch of Perihelion passage	$\tau =$ A.D. 1836·43
Periodic time	$P =$ 182.12 years

and the following formulæ of computation thence resulting:—

$$\tan (v - 46°\ 15') = [0{\cdot}03791] \,.\, \tan (\theta - 5°\ 33')$$

$$\tan \tfrac{1}{2} u = [9\ 40293] \,.\, \tan \tfrac{1}{2} v.$$

$$1836{\cdot}43 - t = [9{\cdot}70408] \,.\, \{u - [1{\cdot}70237] \,.\, \sin u.\}$$

COMPARISON OF THESE FORMULÆ AND ELEMENTS WITH THE TABLE OF MEAN OBSERVED EPOCHS.

t	θc	$\theta c - \theta o$	t	θc	$\theta c - \theta o$	t	θc	$\theta c - \theta o$
	° ′	° ′		° ′	° ′		° ′	° ′
1781.89	131 0	+0 16	1832.32	70 8	− 0 26	1838.31	234 12	+0 50
1803.20	121 2	+1 47	1833.27	60 10	− 1 52	1839.36	216 24	+0 31
1822.25	104 52	+1 28	1834.34	44 24	+ 0 5	1840.41	206 4	+0 22
1825.32	99 6	+2 13	1835.25	20 26	+ 2 13	1841.34	199 50	−0 17
1828.35	90 57	+0 27	1836.31	333 27	−16 31	1842.41	194 33	−0 26
1829.22	87 55	+0 12	1836.41	325 40	− 7 44	1843.33	190 55	−0 42
1830.44	82 24	+0 29	1837.30	269 6	+ 1 41	1845.34	185 24	−0 8
1831.85	77 13	−0 33						

COMPARISON OF THE SAME ELEMENTS AND FORMULÆ WITH THE TABLE OF INTERPOLATED EPOCHS.

θ	$\theta_c-\theta_o$	θ	$\theta_c-\theta_o$	θ	$\theta_c-\theta_o$	θ	$\theta_c-\theta_o$
°	° ′	°	° ′	°	° ′	°	° ′
180	+0 34	50	—0 40	330	—4 8	250	+1 7
120	+1 47	40	+0 29	320	—4 9	240	+0 55
110	+1 59	30	+1 7	310	—4 7	230	+0 59
100	+1 18	20	+1 2	300	—3 22	220	+0 29
90	+0 38	10	+0 28	290	—2 43	210	+0 12
80	—0 6	0	—0 52	280	+0 4	200	—0 9
70	—0 46	350	—2 24	270	+1 38	190	—0 50
60	—1 14	340	—3 30	260	+1 35	185	—0 34

It will not be found easy to represent either the interpolated or the mean observed epochs *throughout* much more closely than by these elements. With regard to the deviation of 16° 31′ in the observed epoch of 1836.31, neither Captain Smyth nor Mr. Dawes are, I believe, disposed to attribute any weight to their observations at that epoch, the stars being then so excessively close that a doubtful elongation was all the indication of its being actually double afforded by their instruments. The Dorpat angle at the perihelion epoch, though stated by M. Struve (*Mensuræ Micrometricæ*, &c. p. 288) to be the result of three days' observations by Messrs. Otto Struve and Sabler, is in fact the mean of two measures of the latter, which, no doubt for valid reasons, he has considered preferable to the others, and indeed the great difference among the individual measures on these three days (which, *allowing for the motion of the stars in the interval*, amounts to fully 12°), clearly shows the difficulty of the observation, and the necessity of exercising some judgment in giving a preference to some one result over the other. Now one of M. Sabler's observations thus preferred gives 329° 42′ for the position at the epoch 1836.41, differing only by 4° 2′ from our computation. The other discordances are not greater than may very reasonably be looked for in the present and past state of this inquiry, even when dealing with the mean results of several observers; and the chief among them (that of 1825.32) would be reduced to little more than half its amount, had M. Struve's observation at that identical epoch been used in place of that actually employed. On the whole showing, therefore, I think it will be readily admitted that the elliptic hypothesis is very satisfactorily sustained. The apparently systematic alternation of positive and negative errors, each prevailing over considerable arcs of the orbit, might indeed be regarded, in a more advanced state of the subject, as indicative of some disturbing cause of a periodical character, but at present such a conclusion would be quite premature.

The allineation of Bradley and Pound of the two stars of γ with the line joining α and δ Virginis in 1718.10 has not been included among the data of our interpolations, which extend only from 1781 to 1845. It will therefore be interesting to see how nearly our new elements represent this remarkable observation. I have already observed that the true angle of position

resulting from this allineation at the epoch in question is 150° 52′ instead of 160° 52′, as, by a mistake of 10° in reading off the angle of position of those two stars, I originally stated it. Now if we calculate the position for that epoch from the elements above given, we find it to be 149° 16′, differing by only 1° 36′ from Bradley's comparison duly corrected for the ocular equation. This is certainly very satisfactory, being within the limits of what we must even now call good micrometrical measurement. The error of the position concluded from the places of the two stars in Mayer's Catalogue for 1756 is — 5° 26′.

Hitherto no notice has been taken of the apparent distances. They have formed no part of the data from which any of the elements above stated have been computed. The following comparison of the measures taken at different epochs and by different observers with those calculated by the aid of these elements for the respective epochs exhibits a correspondence which, under all the circumstances of the measurements, must be regarded as in the highest degree satisfactory, and as completing beyond all reasonable doubt the evidence in favour of elliptic movement.

t	ρ^{o}	ρ^{c}	$\rho^{c}-\rho^{o}$	Authority.	t	ρ^{o}	ρ^{c}	$\rho^{c}-\rho^{o}$	Authority.
	″	″	″			″	″	″	
1781.9	5.63	5.60	—0.03	H. (?)	1833.4	1.18	1.10	—0.08	Sm. Σ.
1803.2	5.91	4.97	—0.94	H. (?)	1834.3	1.07	0.84	—0.13	Sm, h, D, Σ.
1819.4	3.56	3.54	—0.02	Σ.	1835.4	0.51	0.64	+0.13	Σ, Sm.
1822.2	3.32	3.19	—0.13	Σ, Sh.	1836.4	0.26	0.50	+0.24	Σ, (?)
1823.2	3.30	3.04	—0.26	Amici.	1837.4	0.60	0.57	—0.03	Σ, E, M.
1825.3	2.81	2.74	—0.07	Σ, S.	1838.4	0.83	0.86	+0.03	Sm, O, M.
1828.4	2.07	2.21	+0.14	Σ.	1839.4	1.00	1.02	+0.02	Sm.
1829.3	1.79	2.05	+0.26	Σ, h.	1840.4	1.28	1.37	+0.09	O, D.
1830.4	1.90	1.82	—0.08	B, h.	1841.3	1.65	1.61	—0.04	D, M.
1831.3	1.77	1.62	—0.15	D, h, Σ, Sm.	1842.4	1.72	1.86	+0.14	A, D.
1832.4	1.23	1.37	+0.14	D, h, Σ, Sm.	1843.3	1.90	2.06	+0.16	Sm.

In this table ρo denotes the observed and ρc the calculated apparent distance or radius vector of the ellipse. The observations of 1781 and 1803, as well as that of 1836, at the epoch of the perihelion passage, are founded on estimation of diameters, and are therefore necessarily liable to greater error than the others, which all rely on micrometrical measures. In particular, the estimate of 1803 is certainly much too large. That of 1836 is doubtless an under estimate, owing to the peculiar and difficult circumstances of the observation, of which an account may be seen in M. Struve's great work (*Mensuræ Micrometricæ*, &c.). The other errors are all within the limits which the different micrometers, and, above all, the different habits of observers in clipping the stars more or less closely, &c., have hitherto (unfortunately) been found to admit as easily possible, and which render

it, in my opinion, impossible to employ the measurements of distance hitherto recorded, as safe elements of calculation. The semi-axis of the real orbit which these comparisons suppose is 9".69.*

* Since the greater part of these calculations were made, I have received, through the kindness of M. Maedler, the following series of observed epochs: 1841·355, Pos 200° 6′; 1842·361, 196° 11′; 1843·349, 192° 9′; 1844·356, 188° 55′; 1845·367, 186° 57′. These epochs have not been included in our interpolation, and cannot therefore be fairly compared with its results. When projected separately, they exhibit *a systematic and regularly-increasing deviation* from the projection of Mr. Dawes' and Captain Smyth's observations, of the very same nature as that which compelled me to abstain from including among our normal data the Dorpat observations from 1828 to 1834. Facts of this kind go to prove that full confidence cannot yet be placed in *any* micrometrical measures, even of position angles, and in the case of easy stars (as this is once more become); and they lead us to insist on the necessity of an immense accumulation of measures from a variety of observers, and unremittingly continued for a long series of years, as the only ground of hope for the attainment of *accurate* elements of this or any other double star. This communication is accompanied by a set of elements—(the fourth now calculated by this indefatigable astronomer)—placing the perihelion epoch at 1836·29, and assigning a period of 148·78 years. Comparing the orbits which seem entitled to most reliance, it appears certain that the excentricity lies between 0·855 and 0·880, the inclination between 23° and 27°, the perihelion epoch between 1836·20 and 1836·45, and the period between 140 and 190 years. The best defined element is that which is usually, but absurdly, called the place of the perihelion on the orbit (or ☊ + λ in the notation of Art. 192), which cannot differ above a degree one way or the other from 319° 20′. This language ought to be reformed, and the element itself disused, by general consent among astronomers, as a source of endless misapprehension and mistake.

In thus enrolling this valuable document entire, we must remind the amateur that, if bent upon using the graphic geometrical form of investigating the orbits of revolving Double Stars, he must recur to Sir John Herschel's able and ready exposition of the method, as given in the Memoirs of the Royal Astronomical Society (volume v. part i). Besides the first application of the process to γ Virginis, he will find the cases of some other remarkable binaries discussed in detail, with the periods then obtainable from all the available data, as here enumerated:—

α GEMINORUM	Period 252 years.
σ CORONÆ BOREALIS . . .	—— 286·6 ——
ξ URSÆ MAJORIS . . .	—— 69·72 —
70 (p) OPHIUCHI . . .	—— 80·34 - -

NO. IV.

(*Referred to at page* 368.)

AN ORBIT OF γ VIRGINIS.

[Derived from all the Observations between 1718 and 1858. By Captain Henry Augustus Smyth, Royal Artillery.]

For this orbit I have used throughout the graphic method of Sir J. Herschel, according to your recommendation, with such slight modifications as I shall proceed to mention.

In forming the Interpolating Curve, I laid down all the known observations* of position from 1718·2 to 1858·4 on an unusually large scale; and, from an inspection of the latter part of it even as reduced in the Plate, I think it will be evident, that the size allows peculiar facility for making those slight alterations in the curve which are usually found desirable after trying the points obtained from it for an ellipse. (The dotted line represents the curve originally drawn, the continuous line being that which, equally faithful to the observations, was found on trial to afford points more favourable to an ellipse.) I think also that the appearance of this part of the curve, with the cloud of observations so widely scattered about it, will shew the desirability of having, not only plenty of and various observations, but also some scale of reliability, or weight, attached to them all.

In obtaining the distances from this curve, I preferred the system of drawing tangents to it, instead of that of taking its differences, because the former retains the peculiar advantage of the graphic method, in allowing the eye to correct the inaccuracies of the preceding process, viz., in this instance the drawing of the curve. And I applied it, not at any strictly regular intervals of time or angle, but more frequently to the less doubtful parts of the curve, or to such as appeared more influential on after proceedings.

In passing an ellipse amongst the points thus obtained, I arrived at the principal innovation, which appeared to me desirable in some special cases of the method. This consists in testing the ellipse obtained as to the relative proportions of certain sectors of it due to observed intervals of time; of course if the ellipse corresponded with the observations, the proportion of the area of each sector to its observed time would be the same as that of the sum of all their areas to the sum of all the times, or as the area of the whole orbit to its whole period; but in my special case each sector gave a different proportion. I accordingly set myself to shift the position and alter the dimensions of

---

* Namely, those of Bradley and Pound, Cassini and Tobias Mayer, Sir William Herschel, Sir John Herschel, Sir James South, the two Struves, Admiral Smyth, M. Mädler, the Rev. Mr. Dawes, Mr. G. B. Airy, Lord Wrottesley, Mr. Isaac Fletcher, Captain Jacob, &c.

my apparent ellipse, without unduly neglecting any of the points laid down, in order to diminish as far as possible these discrepancies in the results afforded by the different sectors. In my first trial I found that the sector A (see figure of apparent ellipse,) had a smaller proportion to its time, and consequently gave a larger period than the mean of the others; or more particularly that A was too small, B too large, C and D too small, and E about a mean. I then constructed a fresh ellipse, which by being longer should make A larger, and, by being more nearly vertical as to its major axis, should avoid increasing B in the same proportion: but I found that progress in this direction increased the disproportion of C and D in a much greater ratio than it diminished that of A and B, unless I should disregard the restriction of the points laid down about the former region, which I could not afford to do, as it is perhaps the best defined, by observations, of the whole. After trying various other ellipses constructed to favour the sectors sometimes of one region, sometimes of another, I found that they all, with the exception of A, might be made to agree very well among themselves on a period, by an ellipse slightly shorter than would suit A, which I decided to leave out of the question as irreconcileable with the others; and I subjoin the results afforded by the figure finally approved, having in it arrived at a point where further shortening began to make B, C, and D, too small for E:—

Sector A gives for the whole orbit a period of	187·1 years.
B	177·9
C	176·9
D	178·8
E . , ...	177·1;

from which, neglecting A, and giving most weight to the best observed of the other sectors, I conclude the period to be 177·675 years: including A it would be 179·560 years.

This system of comparing the proportions afforded by the areas and times of different sectors, one against another, amounts in fact to a sort of comparison of the obtained ellipse with the whole series of observations, and appears to be more appropriate thus early in the process than when more labour has been spent on a possibly erroneous figure.

The elements of the Apparent Ellipse thereby approved are:—

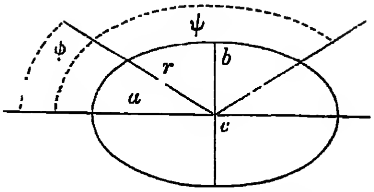
Major semi-axis	3″·867
Position of do.	145° 16′
Excentricity	·839747

Also the resulting radii-vectores—

Maximum distance . , . . .	6″·349
Position at do.	143° 12′
Minimum distance	0″·376
Position at do.	315° 30′

From this apparent ellipse I obtained the elements of the real one, using all Sir J. Herschel's equations, excepting in the case of

$$\cos.\ \phi = \frac{a}{r}\sqrt{\frac{r^2 - b^2}{a^2 - b^2}};$$



where I have considered that when ϕ is very small, any inaccuracy in r is much exaggerated in ϕ; and I preferred deducing ϕ from the measured length of a and of a perpendicular to it, and from ϕ deducing r, by transforming the above equation into

$$r = \frac{a\,b}{\sqrt{a^2 - (a^2 - b^2)\cos.^2\phi}};$$

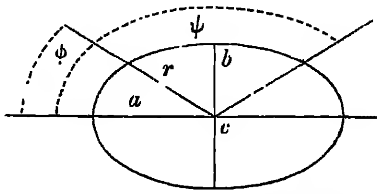
and after due consideration, I obtained the following as the elements of the Real Ellipse;

Major semi-axis	$a = 4''\cdot226$
Position of do.	$\pi = 322^\circ\ 11'$
Excentricity	$e = \cdot88779$
Position of node	$\Omega = 35^\circ\ 37'\cdot5$
Inclination to plane of projection . .	$\gamma = 37^\circ\ 20'$
Distance of perihelion from node . .	$\lambda = 281^\circ\ 42'$
Epoch of perihelion	$\tau =$ A.D. 1836·5
Periodic time	P = 177·7 years

Inasmuch as Ω and λ both appeared to be liable to some uncertainty, I derived the length of a by projection graphically from the apparent ellipse, thus satisfying myself that a and γ were, at any rate, compatible. But in consequence of this uncertainty, I think it would be more convenient, in general comparisons of various orbits of the same star, to have regard only to those elements which, whilst they involve all the indefinite conditions, are themselves definite; to wit, the elements of the apparent ellipse, together with the excentricity and position of the major axis of the real one, and also the epoch of the perihelion and the period. In a word: if our ellipse is to be considered valid, either the date of the perihelion must be consequent on that of the different sectors (which we get from the observations belonging to them), or if we take the date of the perihelion from its own observations the dates of the sectors must be consequent on it: for the drawing of the ellipse represents proportional areas in given times much more closely than do the observations. Now as to dating the sectors from the date of the observed perihelion, I find that it throws them *all* wrong (when compared with the observations) in *one* direction; I am therefore justified in giving them (the unanimous majority) the preference, and dating the perihelion according to its distance of area from them. Then the question arises, from which sectors exactly am I to commence reckoning the areas and times; for, beginning with any one and adding to it the areas (or times) due to the others, most of the others will be slightly altered. I took a sort of mean amongst them in calculating the

From this apparent ellipse I obtained the elements of the real one, using all Sir J. Herschel's equations, excepting in the case of

$$\cos.\ \phi = \frac{a}{r}\sqrt{\frac{r^2 - b^2}{a^2 - b^2}};$$



where I have considered that when ϕ is very small, any inaccuracy in r is much exaggerated in ϕ; and I preferred deducing ϕ from the measured length of a and of a perpendicular to it, and from ϕ deducing r, by transforming the above equation into

$$r = \frac{a\,b}{\sqrt{a^2 - (a^2 - b^2)\cos.^2\phi}};$$

and after due consideration, I obtained the following as the elements of the Real Ellipse;

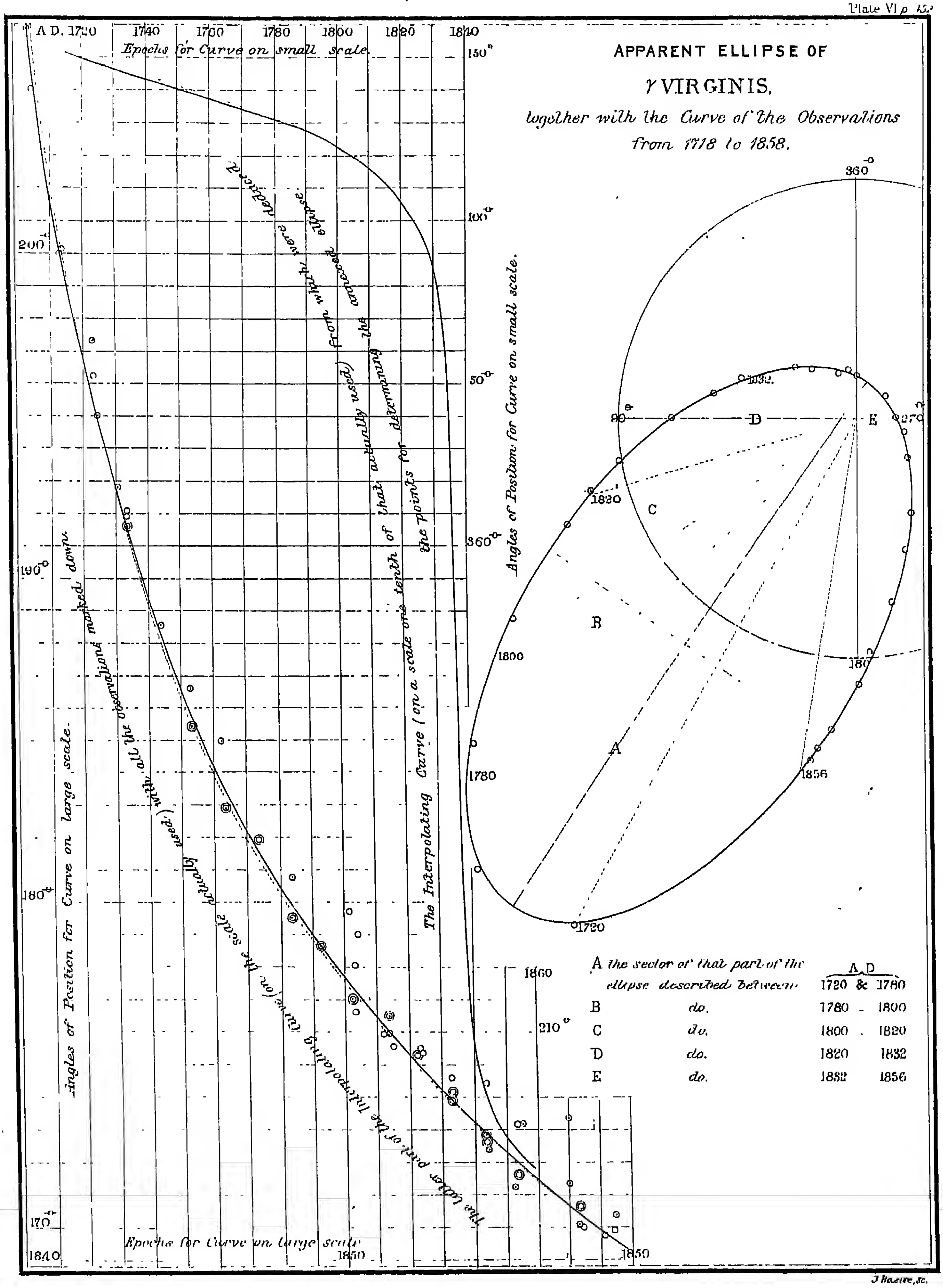
Major semi-axis	$a = 4''\cdot226$
Position of do.	$\pi = 322°\ 11'$
Excentricity	$e = \cdot88779$
Position of node	$\Omega = 85°\ 37'\cdot5$
Inclination to plane of projection . .	$\gamma = 37°\ 20'$
Distance of perihelion from node . .	$\lambda = 281°\ 42'$
Epoch of perihelion	$\tau =$ A.D. 1836·5
Periodic time	P = 177·7 years

Inasmuch as Ω and λ both appeared to be liable to some uncertainty, I derived the length of a by projection graphically from the apparent ellipse, thus satisfying myself that a and γ were, at any rate, compatible. But in consequence of this uncertainty, I think it would be more convenient, in general comparisons of various orbits of the same star, to have regard only to those elements which, whilst they involve all the indefinite conditions, are themselves definite; to wit, the elements of the apparent ellipse, together with the excentricity and position of the major axis of the real one, and also the epoch of the perihelion and the period. In a word: if our ellipse is to be considered valid, either the date of the perihelion must be consequent on that of the different sectors (which we get from the observations belonging to them), or if we take the date of the perihelion from its own observations the dates of the sectors must be consequent on it: for the drawing of the ellipse represents proportional areas in given times much more closely than do the observations. Now as to dating the sectors from the date of the observed perihelion, I find that it throws them *all* wrong (when compared with the observations) in *one* direction; I am therefore justified in giving them (the unanimous majority) the preference, and dating the perihelion according to its distance of area from them. Then the question arises, from which sectors exactly am I to commence reckoning the areas and times; for, beginning with any one and adding to it the areas (or times) due to the others, most of the others will be slightly altered. I took a sort of mean amongst them in calculating the

whole time; but when, in comparing my orbit with the general series of observations, I formed a far larger number of sectors at more frequent intervals of time, I found that my mean amongst the times did not fairly represent the mean amongst those observed, and that it would do no better by making them all 0·1 year later; in fact I arranged the starting point of my time by noticing the whole range of observations instead of what I had used in the earlier part of the process, a limited selected number of them: and if the date be altered in any one part of the ellipse it must be so all round, or the virtue of the principle of "the times being proportionate to the areas" would be lost.

The following is a Table of Positions and Distances from the above ellipse due to various dates; or rather of the dates due to various positions and distances, obtained by weighing, for comparison with the past and with future observations.

Epoch. A.D.	Position. °	Distance. ″	Epoch. A.D.	Position. °	Distance. ″
1722·88	150·2	6·142	1852·60	175·0	3·341
1781·20	131·0	5·647	1854·57	172·8	3·577
1802·90	120·5	4·495	1857·36	170·3	3·859
1818·92	106·6	3·153	PREDICTIONS.		
1822·72	101·2	2·743			
1824·80	97·4	2·494	1861·65	167·0	4·275
1827·73	90·0	2·082	1864·92	165·0	4·518
1830·10	82·0	1·722	1867·34	163·5	4·708
1833·00	65·5	1·224	1870·35	162·0	4·894
1836·14	355·0	·424	1874·44	160·0	5·153
1838·04	231·0	·753	1879·00	158·0	5·365
1839·70	210·0	1·200	1884·27	156·0	5·600
1843·24	191·7	1·993	1889·90	154·0	5·812
1846·65	183·5	2·541	1895·76	152·0	6·001
1849·55	178·7	2·966	1900·58	150·2	6·142


APPARENT ELLIPSE OF
γ VIRGINIS,
together with the Curve of the Observations
from 1718 to 1858.
Epochs for Curve on small scale.
Epochs for Curve on large scale
Angles of Position for Curve on small scale.
Angles of Position for Curve on large scale.
The Interpolating Curve (on a scale one tenth of that actually used) from which were deduced the points for determining the annexed ellipse.
Angles of Position for Curve on large scale, with all the observations marked down
The latter part of the interpolating curve (on the scale actually used)
A the sector of that part of the ellipse described between
B do.
C do.
D do.
E do.
A.D.
1720 & 1780
1780 - 1800
1800 - 1820
1820 1832
1832 1856

J. Basire, sc.

No. V.

(Referred to at page 369.)

A FAREWELL TO THE DOUBLE STAR γ VIRGINIS. AT THE EPOCH OF 1858.

"He that enlarges his curiosity after the works of Nature, demonstrably multiplies his inlets into happiness"

JOHNSON.

I.

Hail glorious Pair—ineffably remote—
  Your lights not borrowed, but inherent shine;
Your mystic orbs in subtile ether float,
  And prove to Man your origin divine:
Pure double-star—bright telescopic view—
  Two suns, a central point gyrating round;
A system binary, with orbit true,
  Its form elliptic, by a period bound:
Perhaps a train of planets decks each sphere,
In endless round throughout your vast career?

II.

Such is the work of that GREAT ENS, of whom
  Man's aspiration forms no notion clear,—
Who holds of countless worlds the awful doom—
  So grand—so vast—it quails the mind with fear:
Sublime is HE, whom no conceptions grasp,
  Much less exalt, or amplify in word:
Avaunt, ye blind that fain his rule would clasp,
  And all his greatness to our globelet gird,
Materializers who—in thought perverse—
Rashly would circumscribe the Universe.

III.

Yes! there are those who, by inverting powers,
Measure celestial realms by our mean clod,
Pronouncing bodies of like weight with ours
As needful to the other works of God;
Earnest to show that physical conditions,
Such as exist on the terraqueous ball,
Must form the system, or exact munitions,
Of intellectual life throughout them all:
This at the threshold is a grievous stumble,
And ought to make the pride of reason humble.

IV.

See upon Earth what marvels meet our gaze—
The human black, the red man, and the fair—
The elephant, the whale, the ant, the rays,
The condors, and the midges in the air!
The eagle wings his flight 'mong solar beams,
And nictitating meets the flood of light,
His visual focus through the medium gleams
On every distance which encounters sight—
Contrast with this the groping mole, purblind,
The duck-bill'd platypus, and the lemur kind.

V.

The trunk-mouth'd, suction-footed, squamose fly,
By many deem'd so low in Nature's scale,
Doth microscopic lens to sight apply—
As the papilio—but unlike the snail.
The promatomus—fish both strange and rare—
Doom'd to th' abysses of the deepest deep,
Has eyes enormous, and contriv'd with care,
The glimmering particles of light to reap.
In all of these what vast design is shown,
For life's existence throughout every zone!

VI.

Then mark their habits, see their varied food—
  Their sustentation on each local spot,
In vales, on hills, in sands, on rocks, in flood—
  Wherever Providence has fix'd their lot.
Not only these, but myriads beside
  Of birds, and beasts, and fishes that migrate,
As well huge mammals in their strength and pride,
  As infusorian atoms, oculate—
So small yet perfect, that with skilful hand,
We make twice-twenty on a needle stand.

VII.

Study the passions of Creation's tribes,
  Observe where instinct doth tow'rds reason draw,
See the affections which each class imbibes,
  And note their wiles and strategy in war:
Men who can view unmoved these wise displays,
  Or see the seasons change without a thought,
Can ne'er their minds to rightful standard raise—
  Their Gloria in Excelsis is as nought;
And should the Atheist ever boast such pass,
'Mong flocks and cattle let him graze the grass.

VIII.

Here is a glimpse of the mysterious Power
  Which fashions all their destin'd path to run,
To live for ages, or exist an hour,
  To dwell in darkness, or to brave the sun;
E'en in ephemera purpos'd schemes are seen:
  Perfection boundless, makes the mind expand,
Yet th' overwise by force would intervene,
  And lock the wheels of progress to a stand.
Not the first time dogmatic Dons have sought
To paralyse intelligence and thought.

IX.

Those recreant Seers once quash'd Copernik's laws,
At Galileo's grand discoveries sneer'd;
And Newton's revelation of the cause
Of movements systematic, basely fear'd—
A dogma which, they said, just doctrine spurn'd,
Since Nature's secrets ne'er should be unfurl'd:
They broke St. Virgil, and poor Bruno burn'd,
For starting antipods, and world on world.
E'en now dark threats, albeit in tone suppress'd,
Prove, though the spirit's scotch'd, 'tis not at rest.

X.

Some wretched saws, by bloated ign'rance flung,
The charge of blunder to large views apply;
'If 'tis a blunder, 'tis an error sprung
From noble root—high thought of the MOST HIGH!'
These addle-headed churls, had they a chance,
Would from the human scan at once efface
The wond'rous art affording us a glance
Of the CREATOR and the *creature's* place,
And in the void restore that dark'ning gloss,
Which holds—'Quæ supra nos, nihil ad nos.'

XI.

Vain men: to deem this speck the only care
Of wise OMNIPOTENCE. Planets around
May also in his endless goodness share—
In many forms of sentient life abound.
Such is, at least, a rational belief,
And observation gives it valid force—
Such to examine is a step in chief
To raise our worship to its highest source:
For glowing intellect can ne'er be found
Better employ'd, than in this course profound.

XII.

One sturdy dolt the telescope decries,
  One sees the sun no bigger than a cheese,
One deems it wicked to aid natural eyes,
  And all stand forth an ignorant horde to please.
'Tis said by Bacon that we—'cannot fly
  Upon the Senses' gaudy waxen wings
To gain the secrets of the Deity'—
  Yet such the rant with which the welkin rings:
Dare they deny that the Almighty's hand
'Could form a world from ev'ry grain of sand'?

XIII.

E'en learn'd Ignotus, 'midst such scenes as those,
  Devoutly bent to tame the wayward mind,
Jests about 'potters' wheels,' and 'fishes' roes,'
  And 'lumps of light' instead of stars can find;
In the dim nebulæ 'whiffs of smoke' he'll see,
  The distant orb is but our system's 'spark,'
While planetary globes of small degree
  Are 'bits' which bungled manufacture mark!
Ah, when a mortal second agents plies,
How quick the Majesty of Causes flies.

XIV.

Such fallacies wild Quid-nuncs love to chase,
  Although no evidence by them is brought;
While circumstance, and our own system's case,
  Support suggestions rear'd by forceful thought.
For, from analogy, can Demus doubt
  That on its axis rotates every sphere—
Or hold that other planets roll without
  The gaseous envelope called atmosphere?
They show their years and seasons to our gaze,
With the allotment of their nights and days.

XV.

All this with sacred lore doth coincide—
Religion's cogent word ne'er swerves from truth—
The Royal Psalmist hints of worlds beside
The one on which he pass'd his age and youth;
Science agrees, whichever way we look,
With reason, and with revelation too,
Nor is there an expression in THE BOOK
To militate against the plural view;
Then why should school-men lead weak minds aloof
By crabb'd assertions, sans a single proof?

XVI.

The Bible *spiritual* matters treats,
And to the thinking being comfort brings;
It rarely dwells on *physical*—or meets
The querist's doubt upon *material* things:
But that the heavens with countless orbs are strewn,
In complicated yet arrang'd position,
Marking God's finger, to our view is shewn—
Though little of their nature or condition:
JEHOVAH there—in type distinct and terse—
Sets forth the Great Book of the Universe.

XVII.

Bright emanations of maturest care
Have roused to anger the contracted mind—
Others disparagement of science swear,
Tow'rds wealth and power an easy road to find;
INTELLIGENCE SUPREME they mystify,
In fierce though unsubstantial argument;
To serve such purpose see the ready lie,
Working in evil way, however meant:
Some have been forc'd to eat their words untrue,
And sorry food they ever found them too.

XVIII.

Fear not, Phil-asters, such control as this,
  Yours is a step towards a good conclusion,
At all events it speaks that mental bliss
  Which scorns to think the LOGOS works confusion.
Nor have the Pundits 'had you on the hip,'
  Albeit they trumpet loudly in defiance;
Keep your course steady like a goodly ship,
  And on ANALOGY place firm reliance;
Your theory of cosmical economy
Rest on INDUCTION, source of true astronomy.

XIX.

O Virgin's gem—meet for astrolatry—
  Gamma, the true *Διγαμμα* of the sky;
O glowing orbs, whose mystic ways to see,
  Full thirty years have occupied mine eye.
Your long ellipse, in intervolved maze,
  Yet under orderly harmonious motion,
Bade, while unfolding to my ardent gaze,
  My thoughts to palpitate with strong devotion:
My soul seem'd bounding from its mundane clod,
To peer, 'through Nature, up to Nature's God.'

XX.

But e'en while struggling in our mortal coil,
  Unbounded mercy through the whole appears,
Thus to permit of Heav'n a glimpse the while,
  And mark a guerdon for our hopes and fears.
By contrast then, how poor all worldly strife,
  The Politician—the ambitious Fool—
The Courtier wedded to a spurious life—
  The toiling drudge for riches, Fortune's tool;
Still poorer they whose days—a num'rous caste—
In idle sensuality are pass'd.

XXI.

To those who with me watch'd yon glorious track,
I owe the strengthen'd purpose of my mind;
Herschel, with zeal, did my exertions back,
Thus acted Airy, Wrottesley, Adams, Hind;
All these sustainers claim my warmest praise—
So worthy Johnson, and friend Fletcher too:
For every aid my grateful thanks I raise,
Since I must bid you, HEAVENLY PAIR, adieu!
And further steps your orbit-course to gain,
I leave to Dawes, to Pogson, and to Main.

XXII.

For now at seventy years—'threescore and ten'—
I cease my nightly vigils in your cause,
Rejoiced t'have plac'd before the eyes of men
My mite to fix the knowledge of your laws:
And while we prove that in those realms afar
Mysterious GRAVITATION holds its sway,
While man with optic pow'r can scan each star
And find their sev'ral data day by day—
Well may we echo what the Poet said,
'AN UNDEVOUT ASTRONOMER IS MAD!'

W. H. S.

St. John's Lodge, near Aylesbury.
19—VI—1858.

NO. VI.

EXTRACT FROM ADMIRAL SMYTH'S ADDRESS TO THE ROYAL GEOGRAPHICAL SOCIETY.

(*Referred to at page* 397.)

"The papers read at these meetings were communicated by Fellows and zealous travellers from all parts of the globe; and, as they consequently relate to its various and particular quarters, I shall take the liberty of citing some of them in connection with those parts in detail. I will therefore at present only express how greatly we have been indebted to the contributors of such documents for the information given, as well as to those gentlemen who favoured us with the results of their actual knowledge—derived either from experience or well-directed inquiry—in the interesting discussions which followed the several readings. These evenings have indeed greatly gratified me, in the obvious proof they have afforded of our possessing steady friends who take a permanent concern in the advancement of the objects, and in the continued prosperity, of the Society.

"Those communications which relate to the important consideration of the instruments essentially requisite for scientific travellers are of paramount interest to a Society of Geographers: and in this light we may class two which were received in the past year; since, however open to practical objections, they are very likely to lead to useful investigation, and consequent valuable results in the instrumental means of measuring the heights of mountains. This has been effected with great exactitude by means of the simple yet powerful Torricellian Tube; and the only objection ever yet made to its use has been its difficulty of transport among abrupt heights.* In probing the revealments of science, it is advantageous, for general argument, to assume certain conditions as being undeniably, if not absolutely, true. Now the mean gravitation exerted upon the surface of the earth by the atmosphere, as indicated by the barometer, equals a column of mercury 30 inches high; that is, the column of air pressing upon the open end of a bent tube filled with quicksilver, exactly balances that quantity which represents a compression of 15 lbs. upon every square inch of surface. Here then is a natural scale for ascertaining the pressure; which pressure is a compound of the weight of the gaseous envelope and the elastic force of the aqueous vapour contained in it. Various contrivances have been from time to time suggested for determining terrestrial altitudes, but no faith can be reposed in any other method except the expensive ones of triangulation and levelling. My own recorded trial of ascertaining the height of Mount Etna in 1814, by means of boiling water and a very sensitive thermometer, was but an experiment.

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* This difficulty I consider to be considerably exaggerated, as is stated in my "Hints to Travellers," published in the XXIVth volume of the Journal of the Royal Geographical Society, page 336. In general, when ascending mountains during my Mediterranean Survey, the barometer was placed—well slung, and with its upper end down—on the back of a steady and careful seaman of my boat's crew.

"The first of the communications alluded to was entitled, 'Remarks on the Use of the Aneroid Barometer,' by Colonel Yorke, late of the Scotch Fusilier Guards, and now your Honorary Secretary; who, during a journey upon the Continent last summer, gave that instrument a very fair comparison with an ordinary barometer under different circumstances and at different heights, the whole of which he has placed before the Society in a tabulated form. From the result of these operations, Colonel Yorke is led to conclude that this instrument may be used satisfactorily when sudden changes of atmospheric pressure are desired to be shown; also to determine differences of level, when it can be compared before and after the observations, and within 24 hours, with a good ordinary fixed barometer. The Aneroid should, previously to being used, be carefully tried with the barometer, at low pressures under an air-pump; and when in use, should always be observed in the same position.

"The second paper on the subject was from Dr. Buist, of Bombay, F.R.S. This gentleman has entered fully into the construction and use of the Aneroid Barometer, and carefully compared it both with the Torricellian Tube and the Mountain Sympiesometer. Among other remarks on the Aneroid's performance, Dr. Buist says, in summing up, 'Should Mr. Adie's surmise prove correct, and the Aneroid at pressures under 28 inches cease, as at present cut, to harmonize with the barometer, it would be well, with an instrument so compact and convenient, to see whether a series of Aneroids could not be so made as to serve in succession for any ordinary elevation; or whether the portions of the scale lower than those on the common Aneroid might not be so altered as to afford the correct pressure. One instrument might serve for the first 2000 feet, a second when only marked up to 28 inches might carry us 2000 feet higher, and so on. The matter might be very easily determined under the receiver of an air-pump, without actual ascent, the barometric gauge with a good scale answering as well as the barometer itself.'

"Now it must be remembered, that after the first introduction of this misnomered* instrument at the Swansea Meeting of the British Association, in 1848, it was so successfully advertised as a perfect means both for meteorological observations and for obtaining differences of level, that many travellers, captivated by its ingenuity of principle and handiness of structure, together with its portability and facility of observing, were induced at once to substitute it for the mercurial barometer. On an examination of the instrument, however, it struck me that these assumptions were too hasty; for, notwithstanding its being so beautifully compact, and its capability for showing average differences, I could not but consider its complexity as an obstacle where a traveller could obtain neither workmen nor comparison, so that injury would be irreparable, and errors might remain without detection. A trial which was made with a friend, Mr. Arthur Kett Barclay, on Leith Hill in Surrey, which is 993 feet in height, was satisfactory; but then, we had a standard barometer to refer to, both before and after the operation. Further inquiry showed me that ulterior improvement is wanting before the Aneroid can be trusted otherwise than as a journeyman to the Torricellian Tube, in the manner of a job-watch to a box-chronometer. But still, if it will only serve for heights of 3000

* I said misnomered, because if M. Nidi, the inventor, derived it from ἀ—νηρὸς—εἶδος, to signify a form without fluid, it does not explain what the instrument is; nor is the better translation very much more acceptable, which renders it—perceivable without moisture.

feet, its use as a travelling instrument is very limited indeed; and we have just seen the adjustment for fresh starts proposed by Doctor Buist. When the atmosphere becomes more rarified than the gas contained in the cylinder, its corrugated surface dilates to its full extent, and ceases to exercise any influence upon the levers and index of the dial plate; so that the latter is liable to become stationary at an elevation of between 3000 and 4000 feet. Hence the indications are very anomalous in extreme cases, the very ones in which a traveller would have most need to use it, namely, in measuring mountains, not hills. Other objections may apply to the moveable index and the perishable materials used within the case, while the scale is nothing fixed in nature, and can never be treated *per se* nor can either the zero point of the scale, nor even the value of it, remain constant.

"I therefore consider it a duty to remind you, that as the machine depends altogether on the accuracy of the experimental trials of the workmen, which you have no hold over, no Aneroid observations can be absolutely depended on, and therefore ought not to be used for any scientific purpose, unless the particular instrument has been tested by comparison with a barometer at three different and distant parts of the scale, before and after the observations."

NO. VII.

ON CERTAIN LOCAL GEOGRAPHICAL CO-ORDINATES.

(*See pages* 173-175.)

Before closing this volume, it may be as well to say a word or two respecting certain topographical conditions of the immediate vicinity, since they are useful to some, and interesting to others. Before coming to what are strictly termed stationary points, it should be stated that the culminating knolls around attain a height of nearly 500 feet above the German Ocean; but, as this is grounded rather upon estimation than standard comparison, the absolute altitude cannot be considered as yet ascertained. The floor of the Market Clock-house at Aylesbury is asserted to be 298 feet above the high-water mark at London Bridge. I know not the authority on which this statement is advanced, but the inquirer for the datum point will find—on referring to the Philosophical Transactions for 1837, page 440—that London Bridge is exactly 10·7544 (or $10\frac{3}{4}$) feet higher than the mean level of the sea.

Those patient readers who have waded thus far with us may remember that, in arranging trigonometrically the position of Hartwell Observatory from the Ordnance station of Aylesbury Church (page 175), I mentioned the rumour which had obtained of General Roy's data requiring further correction. Since that whisper was printed, I applied to the fountain-head for accurate information

on the subject, and have, by the kindness of Colonel Henry James, of the Royal Engineers, been favoured with the following tested determinations:—

Object.	Latitude N.	Longitude W.
Aylesbury church spire	51° 49′ 02″·70	0° 48′ 51″·91
Stone church tower	51° 48′ 10″·53	0° 51′ 46″·95
Dinton church tower	51° 47′ 33″·20	0° 53′ 17″·60
Ellesborough church tower	51° 45′ 09″·15	0° 47′ 17″·97
The ground at Ellesborough church tower .	449·65 feet in height.	
Combe Hill, due east of Ellesborough . .	811·51 ———	

under which correction our former conclusion as to the relative station of the Hartwell House Observatory must now stand as being in

Latitude, 51° 48′ 16″·28 N. Longitude, 0° 50′ 41″·22 W.

This agrees very fairly with scientific inferences derived from observation; and, agreeably to a projection drawn upon graduated paper by inspection, the position of the centre of my residence, St. John's Lodge, ought to be about

Latitude, 51° 47′ 39″·30 N. Longitude, 0° 51′ 19″·80 W.

Such were the normal conditions of the case arrived at by Colonel James's communication; and, as they were quite equal to all possible requirements for local purposes, I was lying pretty contentedly on my oars. But my son-in-law, Captain Henry Toynbee, well known in the nautical world for his zeal in advocating the practicability of obtaining sea-rates for chronometers by means of ⊙ and ☽ and ✱ ☽ ✱, arrived here from a voyage to Madras, on a visit of sufficient duration to admit of an useful expenditure of time. Being very desirous on professional bearings to ascertain —by direct experiment—how close to truth lunar distances taken on shore would approximate to standard points, he undertook to try the problem with a sextant and an artificial horizon. The results which he thus obtained were compared for verification with some theodolite angles subtended from a carefully measured base-line of 300 feet, in an adjacent field to the westward, and then carried to the station of the Ordnance Survey, or grand triangulation of the kingdom. The following is his report to me of the consequent results, and is dated 17th December, 1859:—

At your request I have drawn up the following results of my observations with the sextant on shore, and have also trigonometrically connected them with Stone Church and the Vicarage, thereby comparing my position of St. John's Lodge with the Trigonometrical Survey. These are the steps taken—

LATITUDES OF ST. JOHN'S LODGE.

By Mer. Alts. of the ⊙.

Oct. 5th, 1859	51° 47′ 40″ N.
„ 6th, „	51° 47′ 47″·3
Nov. 24th, „	51° 47′ 35″
Dec. 7th, „	51° 47′ 45″·9
	4) 190 48·2
Mean of 4 ⊙s.	51 47 42·1 N.

By Alts. of ✱s.

Oct. 5th.	By meridian alt. of Marcab.	51° 47′ 9″·2 N.
„ 17th.	By mean of 5 alts. of Polar ✱	51° 48′ 10″·4
Dec. 10th.	By meridian alt. of Marcab.	51° 47′ 8″
„	By mean of 5 alts. of Polar ✱	51° 48′ 15″
		4) 190 51·6
	Mean of 4 ✱s. . . .	51 47 42·9 N.

No observations have been rejected: these eight are the only sights I have taken for the latitude up to the present time, and the results require a few remarks. The sextant used is a good one by Cary; the agreement in the ⊙ latitudes may be accounted for by the same screens being used to find the index-error of the sextant as were used in measuring the altitudes of the sun, whereas no screens were used when taking the altitudes of stars; therefore the index-error found with screens is not applicable to observations taken when no screens are used.

I did apply the index-error found with screens to the star altitudes, and the effect was to make the angle measured 1′ 10″ more than truth; but it was annihilated by picking stars on opposite sides of the meridian. This supports the opinion I have long maintained, that each screen affects the angle measured differently. Marcab's alts. give nearly the same latitude on both nights, as do the North Star's; and their mean agrees well with the ⊙.

When no screens, or very light ones, are used, the index-error of the sextant, *in this state*, may be found by bringing the direct and reflected images of a star to overlap.

Regarding the longitude, I shall now proceed to give the results of my lunar observations at St. John's Lodge; which are expressed in differences of time:—

	⊙ E. ☽
	m s
Oct. 21st, 1859	1 57·8 West.
,, ,, ,,	2 00·6 ,,
,, 22nd, ,,	1 57·7 ,,
,, ,, ,,	1 55·3 ,,
	4)7 1·4
Mean of ⊙ E. ☽	1 57·9 West.

	⊙ W. ☽
	m s
Nov. 28th, 1859	3 51·6 West.
,, ,, ,,	3 41·5
	2)7 33·1
Mean of ⊙ W. ☽	3 46·6 West.

	✱ E. ☽
	m s
Nov. 30th, 1859	1 43·9 West.
,, ,, ,,	1 39·7
Dec 1st, ,,	1 31·6
,, ,, ,,	1 47·5
Dec. 3rd, ,,	1 37·0
,, ,, ,,	1 25·0
	6)9 44.7
Mean of ✱ E. ☽	1 37·5 West.

	✱ W. ☽
	m s
Dec. 3rd, 1859	7 17·0 West.
,, ,, ,,	7 13·3
Dec. 8th. ,,	5 46·2
,, ,, ,,	6 19·3
	4)26 35·8
Mean of ✱ W. ☽	6 39 West.

From the above I derive the following general mean:—

	m s
Mean of ⊙ E. ☽	1 57·9 W.
Mean of ⊙ W. ☽	3 46·6 W.
Mean of ✱ E. ☽	1 37·5 W.
Mean of ✱ W. ☽	6 39 W.
	4)14 01·0 W.
Longitude of St. John's Lodge by the mean of ⊙ and ✱, E. and W. of ☽	3 30·3 W.

Hence I have the following position for St. John's Lodge:—51° 47′ 42″·5 N., 0° 52′ 30″ W.

Accompanying this is a projection of the base-line and angles measured at your suggestion, by which I experienced the admirable facility and utility of the theodolite. To make my endeavour more complete, the following positions of St. John's Lodge, and its relative differences of level, are given:—

	Latitude.	Longitude.
By trigonometrical reduction	51° 47′ 39″·30 N.	0° 51′ 19″·8 W.
By my observations with the sextant	51° 47′ 42″·5	0° 52′ 30″ W.

The lower part of the weathercock of Stone Church is 72ft. 5in. higher than the chimney of St. John's Lodge.

The chimney of the Vicarage is 44ft. higher than the chimney of St. John's Lodge.

The weathercock of Dinton Church is 35ft. 6in. higher than the chimney of St. John's Lodge.

It must be said in conclusion, that the above individual "lunars" are not so satisfactory as those which I have taken at sea, and I am therefore led to think that lunars give more correct results when taken in variable lower latitudes than in a fixed high latitude during the winter. This remark is most applicable to sun lunars; but the steady agreement between the single lunars on one side of the moon show what correct rates to chronometers may be obtained after the lapse of a month: and the mean of all the lunars taken (for not one has been rejected) agrees to a mile with the true longitude.

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